THURSDAY, NOVEMBER 21, 1889.

ROCK METAMORPHISM.

Chemical and Physical Studies in the Metamorphism of Rocks, based on the Thesis written for the D.Sc. Degree in the University of London, 1888. By the Rev. A. Irving, D.Sc.Lond., B.A., F.G.S. (London: Longmans, Green, and Co. 1889.)

DR. IRVING is well known as a writer on Bagshot beds. He appears in a new light as the propounder of theories dealing with the metamorphism of rocks. His ideas on this subject are classified under three heads: paramorphism, metatropy, and metataxis. *Paramorphism*, according to the author, includes those changes within in the rock-mass, involving changes in the chemical composition of the original minerals and the formation of new minerals; *metatropy* denotes changes in the physical character of rock-masses; and *metataxis*, mechanical changes, such as the development of cleavage. Changes brought about by the introduction of a new, or the removal of an old mineral (*e.g.* dolomitization) are treated under the head of *hyperphoric change*.

The author writes, he tells us, for those who are willing to look at geological phenomena "in the light of physical and chemical ideas." To all others his dissertation "must read rather like romance than sober science." He is not far wrong when he complains that the chemical side of geology has been neglected since the time of Bischof. The reason for this is to be found in the fact that geologists have been too busily engaged in reaping golden harvests in the demesnes of palæontology and stratigraphy to be much tempted by the allurements of chemical geology. With the resuscitation of petrology, however, the chemical constitution of rocks begins again to present problems of great interest and importance. But the author turns his chemical knowledge to bad account, we think, in applying it to the elaboration of sweeping generalizations. The views he puts forward may or may not be founded on sound chemical and physical axioms; but mere test-tube reactions will not suffice to explain the operations of Nature in the vast laboratory of the universe. The phenomena of metamorphism represent the net result of numerous and often antagonistic forces; and are not always simple reactions that may be expressed by a neat chemical equation.

Dr. Irving appears to be highly gifted with what he terms a "scientific imagination," the meteoric flights of which carry him far above the solid ground of fact or even justifiable theory. An instance of this faculty of the author's will be found on p. 66, where he seeks to explain the origin of foliation in Archæan rocks by the influence of "solar and lunar tides upon the non-consolidated magma in the Archæan and pre-Archæan (*sic*) stages of the earth's evolution." He proceeds:—

"In such an unequally viscous mass there would be tension, contortion, and shearing to any extent during the tidal pulsations which the magma was suffering. . . . Portions already solidified, or nearly so, by segregation or otherwise, as time went on, would by their *vis inertiæ* present obstacles around which a fluxion structure would develop itself in the contiguous portions of the yielding magma, giving us perhaps in some cases 'Augengneiss.' The local tension of parts of the viscous lithosphere, especially near the crests of the waves, would imply stretching and consequent lowering of temperature, a circumstance favourable to local solidification. Who shall say that in the later and feebler struggles of this kind, as secular cooling went on, and the magma approached nearer and nearer to the conditions required for consolidation, some of these tidal waves may not have become *in situ* sufficiently rigid to outline some of the earliest lines of elevation?"

This is speculative enough in all conscience. On p. 29, the author discusses the influence of the salts dissolved in sea-water on submarine lava-flows, and suggests that serpentinization and the conversion of orthoclase into albite are the result of some process of "submarine paramorphism" effected by this agency. This, again, is pure hypothesis, there being no facts to support such a view.

There is a flavour of pedantry in the use of such expressions as "burnt hydrogen" for water (p. 64), or in such sentences as "orthoclase is probably the embryonic silicate of the terrestrial lithosphere" (p. 67). As the old lady is said to have remarked of the word Mesopotamia, there is something especially comforting and satisfying about this last sentence.

The pages bristle with "hard words," some of which are new to science. "Vitreosity" has an uncanny sound; "apophytic" is curious; and "dehydrodevitrification" is as inelegant as it is long. Indeed, so technical is the author's language that a clear understanding of his meaning involves constant reference to his definitions. Unfortunately such reference is rendered impracticable by the absence of an index.

The book bears witness to Dr. Irving's extensive acquaintance with foreign chemical and geological literature; references to foreign sources being abundant, sometimes superfluous. Indeed, there is more evidence of the author's acquaintance with literature than with facts derived from original observation. Good ideas may here and there be picked out; and the work no doubt contains some plausible explanations of geological phenomena; but of this we are assured, that the science of geology will not be advanced by those who spend their time in manufacturing wide-reaching generalizations or attractive theories in the library, but rather by those who are content to labour, with the hammer in the field, the microscope in the cabinet, and the balance in the laboratory at the ofttimes wearisome task of unravelling details.

This book may be placed in the same category as Sterry Hunt's "Chemical and Geological Essays." Such books can be recommended to those with a taste for speculation and rumination. To others they may be productive of mental confusion and headache.

HAND-BOOK OF DESCRIPTIVE AND PRACTICAL ASTRONOMY.

Hand-book of Descriptive and Practical Astronomy. By G. F. Chambers, F.R.A.S. Part I. The Sun, Planets, and Comets. (Oxford: Clarendon Press, 1889.)

THE avowed aim of the author of this work, since the publication of the first edition in 1861, has been to keep its pages up to date—to make it a sort of *vade mecum* to astronomers; and, regarded as a book en-

deavouring to effect a compromise between purely elementary works on astronomy and advanced treatises, it is worthy of some praise. With the many remarkable developments of astronomical science during the last quarter of a century, the bulk of the original volume has been somewhat increased by additions, and it has now been decided henceforth to publish the work in three divisions, viz.—

(1) The sun, planets, and comets.
(2) Instruments and practical astronomy.
(3) The starry heavens.

The first division of the work is now before us, and viewed as a handy book of reference it has many commendable features; but all that could be said in its praise would be the reiteration of comments upon former editions.

The most important application of spectroscopy to astronomy is too well known to need any enlarging upon. It may be said to be almost entirely a creature of the last quarter of a century, but by far the greater amount of this spectroscopic work has been directed to the sun, whilst many new and important discoveries have been made in connection with it. In pre-spectroscopic times a spot on the sun was only that, and nothing more; and a solar prominence was a stupendous flame, the observation of which was only possible at eclipses. Nothing was known of their constitution; and, in fact, all we now know of the physical and chemical condition of the sun has been gained by spectroscopists. However, it is not necessary here to consider the enormous work that has been done in this direction, but it is our duty most emphatically to protest against a compilation such as the one before us—purporting to be a completely revised account of astronomical labours and advances, and yet rendering terribly conspicuous by its absence everything that relates to spectroscopy. It is like a book on locomotion leaving out all about railways because they were not prominent when the first edition was published. The pictorial representations of the corona, the solar prominences, the surface of the sun and the spots upon it, are well discussed in their respective sections, but no room has been given to an examination of their constitution by means of the spectroscope; and indeed, as far as this book is concerned, the whole work that has been done in connection with solar physics might have been left undone.

But these remarks apply not only to the chapters relating to the sun; those on the planets and comets respectively are in the same incomplete condition. Without the spectroscope, the source of luminosity of a comet was far beyond human ken, and its whole constitution was a matter of considerable doubt; with this instrument, however, much has been added to our knowledge—the comet's light has been analyzed, and the whole sequence of changes, as it goes from aphelion to perihelion and back again, is now understood. Yet the spectroscope might never have been turned to these bodies, or indeed utilized in any way, if the utility and importance of the work done were measured by the brief notice with which the author has seen fit to dispose of it, and the following may be said to be the reason for his grievous omissions:—

"The study of the sun has during the last few years taken a remarkable start, owing to the fact that, by the aid of the spectroscope, we have been enabled to obtain much new information about its physical constitution. The subject being, however, a physical rather than an astronomical one, and involving a great amount of optical and chemical details, it cannot conveniently be discussed at length in a purely astronomical treatise, though something will be said concerning it later on in the portion of this work dedicated to spectroscopic matters."

This explanation, however, only aggravates the fault. The importance of the work that has been done is assented to, but, instead of including that part of it relating to the sun in a chapter on that body, instead of considering the spectroscopy of comets as inseparable from a chapter devoted to their discussion, the author has relegated the whole work to an unpublished section devoted to astronomical instruments. Such an arrangement is undoubtedly wrong. A chapter on the sun must contain all that is known about that body, if it strives to be at all complete; similarly, a chapter on comets cannot approach completion unless their spectra are considered; thus this work cannot lead the general public to a just appreciation of the many advancements that have been made. The most elementary text-books rightly include the spectroscopic labours and discoveries, whereas this so-called hand-book, although aiming at being an historical account of the work that has been directed to the sun, planets, and comets respectively, leaves a vast array of facts out of consideration altogether.

There are a few minor faults, one of which is the figure relating to Foucault's pendulum experiment for determining the rotation of the earth. The author appears to have discarded the method of suspension adopted by Foucault, and the pendulum is sketched as if rigidly attached to a beam. The accompanying text also leaves this most important experimental detail out of consideration.

But apart from these points, the work is worthy of some commendation. An addition has been made to the chapter on comets, viz. a method of determining the elements of the orbit of a comet by a graphical process. The catalogue of comets whose orbits have been computed has also been brought up to date, and similar additions have been made to the chapters on periodic and remarkable comets. Doubtless the book will prove to be what it has been heretofore—a handy reference to some astronomical facts.

ELECTRICAL UNDERTAKINGS.

Proceedings of the National Electric Light Association at its Ninth Convention, 1889. Vol. VI. (Boston, Mass., U.S.: Press of *Modern Light and Heat*, 1889.)

WE have before us, in this volume, an account of the proceedings of the National Electric Light Association in the United States during the Convention held at Chicago on certain days in February 1889.

This body is one which, in the United States, has been brought into existence by the growing necessities and rapid expansion of the electric light and power industry. Probably its nearest English analogue is the Iron and Steel Institute. It is essentially a commercial association, and its aims may be said to be comprised within the limits of the exchange of practical information

amongst its members, and of such joint action as will further the use and success of these electrical trades. Hence its objects are not, exactly speaking, scientific, at least in the usual sense of the word, and the intermixture of genuine desire to exchange veritable experience, with a certain element of effort to push into notice particular personal "interests," renders a discriminating mind necessary in dealing with its Reports. At the time of writing, when the work of practically providing London with distributed electric current is being carried on with energy in diverse directions, and the various Electric Supply Companies are laying down mains and establishing stations, this Report serves a useful purpose of enabling us to judge the present state of the industry in the country where, of all others, it has had the most unhindered development.

In his opening address, the President, Mr. S. A. Duncan, gave some figures which are significant of the immense extent to which the electric lighting business has now progressed in the United States. The total number of arc lights in daily use is about 220,000; of incandescent lamps, some 2,500,000. There are approximately 5700 central stations and isolated plants, supplying electric current to single buildings or groups, or sections of towns. There are 53 electric railways in operation, and 44 in progress, on which 378 electric tram-cars travel over 294 miles of track. The total capital employed and sunk in these various undertakings is probably not under fifty millions sterling. When we consider that this is the growth of ten years, we are bound to admit, not only that this youngest of the applied sciences is of vigorous growth, but that its commercial basis must be sound. The Proceedings of the Convention take the form of a series of Reports on various points of interest which are drawn up by individuals or Committees, and then discussed by the whole body.

One of the important questions which in this meeting received consideration was that of underground conductors. It has been evident for a long time that arc-light wires, telephone, telegraph, fire-signal, and incandescent-lamp wires cannot be permitted to increase without limit in the form of overhead conductors. In the early days of the telephone and arc light the inconvenience of overhead wires did not present itself as a formidable one; but, with their rapid growth, the dangers to life and property arising from an indiscriminate collection of electric wires strung on poles or attached to roofs in large cities became apparent. Hence has arisen a demand that they shall be put underground.

Unfortunately this is not so easy in practice as it seems. The distributing companies in many cases desire to avoid the cost of making the exchange in those cases in which they are operating overhead wires. The expense of an underground system of conductors is from five to ten times that of aërial lines. Moreover, the various methods suggested for sub-laying the conductors in streets and roads have all peculiar merits and demerits. Mr. Edison, as is well known, places the copper conductors in steel pipes, insulating them with a bituminous compound, and lays these like gas-pipes in the streets. This system has been operated for years in New York, Milan, Boston, and Chicago, with a high degree of success. Other inventors have advocated a conduit system; others, again, the use of bare copper conductors insulated in a subway. It is thus seen that the necessary experience for satisfactorily laying down underground systems of conductors for the conveyance of large electric currents is only slowly being collected.

The city of Chicago has one of the most completely developed systems of underground conductors for arc-light wires. There are some seventy-eight miles of underground cable conveying currents under a pressure of 1000–1800 volts. The members of the Convention not unnaturally exhibited considerable differences of opinion on this question of underground conductor systems. A Committee appointed for the purpose had issued a circular to about 1066 managers of central stations and lighting systems and others, with the object of eliciting their opinions on the subject of underground conductors. Out of this number 130 returned very full answers to the various questions, and the diversity of opinion seems very great. It is difficult, however, to believe that the process of collecting information was that which would lead to the best results, and although the various views put forward in the discussion on the Report are interesting, they do not indicate a solidarity of opinion on any one point. It is perfectly certain, however, that in England electric conductors for systems of town lighting by electricity will have to be placed underground, and it is also equally certain that those responsible for this work will have to exercise the greatest discretion and take the fullest advantage of existing experience. The question of the fire risks of electric lighting also occupied the attention of the members. In the United States, as with us, the opinion based on experience is that when the work of installing the electric light is carried out under all known proper precautions, and by the best guidance, there is greater safety in it than in gas illumination, but that when these known precautions are disregarded then danger ensues. Minor questions, such as the disruptive discharges in lead cables and fuel oil, attracted briefer attention. The importance of such a gathering in guiding the experience of those who are fostering an industry like that of electric lighting, in which invention advances by leaps and bounds, is very great. We in England, thanks to the revision of the Electric Lighting Act, are now entering on a period of great electrical activity, and already it has been found that the commercial side of electrical engineering requires the association of those engaged in it for mutual advice and joint action, and the London Chamber of Commerce has now an active Electrical Section which fulfils to some extent the functions of the National Electric Light Association in America.

J. A. F.

DIANTHUS.

Enumeratio Specierum Varietatumque Generis Dianthus. Auctore F. N. Williams, F.L.S. Pp. 23. (London: West and Newman, 1889.)

ONE of the things most wanted by species-botanists at the present time is a set of monographs of a number of the familiar large genera of Polypetalous Dicotelydons. The natural orders of Polypetalæ were

monographed by De Candolle in the "Prodromus" between 1824 and 1830, and the scattered material relating to many of the orders and genera has not since been brought together and codified. As instances of genera now involved in great confusion for want of a more recent elaboration, we may cite Ranunculus, Viola, Papaver, Alyssum, Draba, Dianthus, Geranium, Galium, and many others. The present paper is, unfortunately, not a monograph of Dianthus, but only a list of the known species classified into groups, accompanied by general remarks on the structure of the different organs in the genus, and on their range of variation, so that, though it is interesting and useful as far as it goes, it still leaves very much to be desired. Although, on the one hand, Caryophyllaceæ are dried for the herbarium very easily, and suffer little in the process, yet Dianthus is a very difficult genus for botanists to deal with and to understand. There are 230 species for a monographer to characterize. The range of variation between the extreme types is not great, and some of the commoner species (*e.g. D. Seguieri*, *plumarius*, and *Carthusianorum*) are very variable, the consequence being that, one often sees them named in gardens very incorrectly, forms of *plumarius* especially, which is hardy and spreads readily, doing duty for many totally distinct species.

Dianthus is a genus quite characteristic of temperate and sub-temperate climates. It has its head-quarters in Europe and Western Asia. There are several species at the Cape; a few are Himalayan, Chinese, and Japanese; none reach Australia, New Zealand, or the Andes; and only one just touches the extreme north-western tip of the American continent. There are two principal sub-genera: Caryophyllastrum, of which the carnation may be taken as the type, which is far the largest; and Armeriastrum, or Carthusianastrum, of which the flowers are numerous and clustered, as in the sweet-william. There is a third small sub-genus, intermediate between Tunica and the true pinks, which is classified by Bentham and Hooker with Tunica, and by Mr. Williams, following Linnæus and Koch, as a third sub-genus of Dianthus. Within the bounds of the genus, Mr. Williams finds his primary characters—those which mark groups—in the form of the calyx, the nature of the margin of the lamina of the petals, the presence or absence of a beard at the junction of the blade and claw of the petals, filaments, and styles, the shape of the leaf, and the disposition of the flowers; and his secondary characters—those which distinguish species—in the number and shape of the bracts of the epicalyx, the form of the lamina of the petals and their apposition, the character of the calyx-teeth, the form and structure of the capsule, the form and structure of the seeds, and the disposition of the fascicles of veins in the leaves of the barren shoots and flowering stems. His groups and species do not differ materially from those given in his paper in the *Journal of Botany* for 1885, p. 340. The list would have been more useful if he had stated the native country of each species, and added a reference to where it was first described. We hope, however, that he will see his way to publish, before long, the monograph of which this is a mere outline sketch.

J. G. B.

OUR BOOK SHELF.

Magnetism and Electricity. By Arthur W. Poyser, M.A. (London: Longmans, Green, and Co., 1889.)

SINCE the amount of knowledge that is supposed to constitute an elementary scientific education increases every year, there is sufficient justification for the publication of a series of science manuals designed to meet the growing requirements of the Science and Art Department examinations, and this work is an excellent representation of such a series. Apart, however, from the value of this book as an examination manual, it possesses considerable merit. The matter contained in it is just about as much as would cover the course usually taken in a year's school work; the explanatory text is couched in the clearest language, and the experiments described are capable of being easily brought to a successful termination. Also the 235 illustrations will be of considerable assistance to the student, whilst the many exercises and examination questions interspersed throughout the book may be useful tests of his knowledge. The text-books that in their day have been eminently successful, if unrevised, must be supplanted by others which take a more extended view of the subject; hence it is that this book will compare most favourably with any written for the purpose of imparting a rudimentary knowledge of magnetic and electrical phenomena and the laws by which they are governed.

The Engineer's Sketch-book. By Thomas Walter Barber. (London: E. and F. N. Spon, 1889.)

ENGINEERS and draughtsmen generally keep note-books in which are jotted down most things they wish to particularly remember, accompanied by rough sketches when necessary. The author of this book is no exception to the rule. He tells us he has made many notes and sketches during his experience as an engineer, and has often found the want of such a collection for reference. This volume consists of about 1936 sketches, classified under different headings, of devices, appliances, and contrivances of mechanical movements. The book is certainly unique in its way, and will prove useful to those who have machinery to design, who may require suggestive sketches of mechanical combinations to accomplish some desired end. The author truly remarks that a sketch properly executed is to a practical man worth a folio of description. Hence the descriptions given are generally mere names, with occasionally a concise statement of purpose. Each sketch bears a number, and on the opposite page this number is to be found with the description, &c.,—a very good arrangement.

These sketches are clearly printed, and are probably executed from scale drawings in most cases. Taken as a whole, they fairly represent what they profess to do. Sketch 1636, however, is supposed to represent a Ramsbottom safety valve, but it gives a radically wrong impression of this valve. The lever is shown resting on the two valves certainly, but the spring is attached to the lever at a point considerably above the assumed straight line joining the points resting on the valves—an impossible position. Again, one of the two points of the lever resting on the valves is usually loose and connected with the lever by a pin. The sketch shows the lever and the two projecting points made solid. This example is the most unpractical sketch discovered in the book, and should be rectified in a future edition. A fairly good index adds to the usefulness of the volume. There is ample evidence of careful work on the part of the author, and he is to be congratulated on writing a book which will probably be of use to many engineers and those connected with the profession.

N. J. L.

A Life of John Davis. By Clements R. Markham, C.B., F.R.S. (London: George Philip and Son, 1889.)

THIS is the first volume of what promises to be a series of great value and interest. The object of the series, as explained by the editors, is to provide a biographical history of geographical discovery. Each of the great men who "have dared to force their way into the unknown, and so unveiled to us the face of mother earth," will form the subject of a volume; and an attempt will be made, not only to present a vivid picture of the character and adventures of these heroes, but to estimate exactly the scientific value of their work. If the scheme is carried out in a manner worthy of the stirring tales to which it relates, the series will be a source of much wholesome pleasure to all who care to understand how our present knowledge of the earth's surface came to be built up, and who are capable of appreciating the splendid qualities, moral and intellectual, of all who have won for themselves a place in the list of illustrious explorers. The subject of the present volume could not have been intrusted to a more suitable writer than Mr. Clements Markham. He tells in a simple and natural style the tale of Davis's life, displaying at every stage of the story full and accurate knowledge, and summing up clearly the achievements which entitle the discoverer of Davis Straits to be ranked "among the foremost sea-worthies of the glorious reign of Queen Elizabeth." Two admirable chapters are devoted to the following-up of the work of Davis, and in an appendix the author gives all necessary information as to authorities. Mr. Markham has done his work well, and it will be no easy task for the writers of the succeeding volumes to maintain the series at the same high level.

The Brook and its Banks. By the Rev. J. G. Wood. (London: The Religious Tract Society, 1889.)

The Zoo. Second Series. By the Rev. J. G. Wood, (London: Society for Promoting Christian Knowledge, 1889.)

THE first of these two books was written for the *Girls' Own Paper*, and a few chapters of it have been printed in that periodical. Now the complete work is issued separately, and it will no doubt be welcomed by many readers who have already profited by the late author's well-known writings. The reader is supposed to be conducted along the banks of an English brook, and to learn, as he advances, the characteristics of the living creatures which are to be found by the way. The idea is carried out brightly, and—we need scarcely say—with ample knowledge. There are many illustrations, and they add considerably to the interest of the text.

"The Zoo" contains an account of animals of the weasel tribe, the seal tribe, the rodent family, and various kinds of oxen. The descriptions are clear, compact, and lively, and cannot fail to interest the young readers for whose benefit the book was originally planned. Mr. Harrison Weir contributes a number of excellent illustrations.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Protective Coloration of Eggs.

THE following letter records a very interesting observation which is new to me, and I should be glad to hear if any similar fact has been noted before. If not, it would be very interesting if those who have the opportunity would, in the coming spring, seek for as many nests as possible of the red-backed shrike, and see if they can find any correlation between the colours of the eggs and the lining material of the nest.

Parkstone, November 1. ALFRED R. WALLACE.

"*Merchant Taylors' School, Crosby, Liverpool,*
"*October* 15, 1889.

"DEAR SIR,—I wish to bring before your notice an observation of mine relative to the purpose of colour in animals.

"The red-backed shrike (*Lanius collurio*). Colour of eggs—*either* pale blue or green, white ground with zone of spots at larger end; *or, pink ground* with reddish spots.

"*Observation.*—The colour of the lining substance of the nest—such as roots—assimilates to the colour of the eggs, being dirty gray material when the eggs are to be pale (blue or green) white, but being of red-brown roots, &c., when the eggs are to be pink.

"Evidence for above statement. About sixteen years ago I was a lad of fifteen, an enthusiastic birds'-nester, living at Maidstone, and found several (I forget how many) nests, and noticed this; and it so puzzled me—because I could not make out how the bird knew what coloured lining to select, because she made her nest *before* she laid her eggs—that I have never forgotten it. In those days I had never heard of 'The Origin of Species,' nor did I trouble myself about evolutionary theories, knowing nothing about them, so that there was no *predisposing cause* in me to make a wrong observation. Yet I remember it was only a *school-boy's observation*, and therefore it needs confirmation.

"*Assume the fact.* Protective, obviously. Yet, how does the bird know? We know birds build nests from *observing* other nests, and not by instinct wholly.

"(*a*) Have we here incipient species, in which the young, emerging from *pink eggs*, remember their own infancy in a reddish nest?

"(*b*) Has the sight of the red lining an influence over the mother to tinge the eggs pink—*i.e.* would a shrike brought up in a *pink cage* be more likely to lay pink eggs? or a gray rabbit in a black or white hutch have a greater proportion of black or white variants in her litter?

"(*c*) A mere coincidence; too few observations.

"Will you forgive one who intends to be amongst your audience on October 29 and 30, if not prevented, thus trespassing on your time—time which, spent in research, is so valuable to the whole scientific world? Yet, I *do think* my boyhood's observation is worth recording, if only to direct other observers.

"*E.g.* has the amount of white quartzite veins in a cliff, or chalk, *any* influence in the percentage of white, as against blue, eggs of the common guillemot?

"Believe me, yours faithfully,
"(Rev.) FRED. F. GRENSTED."

Science and the India Civil Service Examinations.

THE position of science candidates in the Civil Service competitions is largely in the hands of the science examiners. In some cases they have practically struck their subject out of the schedule by requiring, or by acquiescing in, the demand for a standard of knowledge far beyond the proportion of marks assigned. Even in the last India Civil Service competition the first two men in chemistry only scored 196 and 195 respectively, whilst the first two in German, out of the same maximum, gained 359 and 353. If the eminent men of science who undertake these examinations would see that science had fair play, many more candidates would be encouraged to study it. Whatever the private views of the Civil Service Commissioners may be, their absolute justice and honourable impartiality are unassailable. Even if they did not altogether concur in the opinions of the examiners, they would give their arguments careful consideration, and see that all interests should be duly regarded.

It will not advance the claims of science to weight them with the very doubtful proposition that "the Universities of England and India" are the only places where "well educated" men are to be found. Many most distinguished men of science have not had the advantage of a University degree in early life. No one would venture to class them for this reason in "an inferior order of men." HENRY PALIN GURNEY.

2 Powis Square, W., November 15.

The Physics of the Sub-oceanic Crust.

In the new edition of his "Physics of the Earth's Crust," Mr. Fisher has made a great advance on his former position, for he sees his way to explain the formation of mountain chains, and all the phenomena of compression which are so strikingly exhibited in the crust of the earth, without depending on his former theory of columnar expansion, and without falling back on the contraction hypothesis.

He believes that the existence of a liquid substratum beneath a thin crust is consistent with the physical conditions of the universe; and argues that no appreciable tide would be produced in it if the liquid magma consisted of an intimate association of fused rock and dissolved gases. He further concludes that this magma is not an inert or motionless liquid, but one in which convection currents are constantly bringing up heat from below, and leading to frequent internal displacements of mass.

In this hypothesis he finds a means of explaining the movements of the earth's crust. Whether Mr. Fisher's position can be maintained must be decided by those who are accustomed to deal with the physical problems involved, but geologists will be glad if it should prove that the objections to the existence of a liquid substratum have been successfully met, for they have always found a difficulty in explaining geological phenomena without having recourse to the supposition of a liquid layer.

One of the most important chapters in the book is that on the sub-oceanic crust, and it is on this that I propose to offer a few remarks, taking it for granted that a truly liquid substratum with a play of convection currents does really exist.

Mr. Fisher's object is to ascertain the thickness and density of those parts of the crust which lie beneath the oceans, and to see whether in these respects they differ from the continental portions. This he does by making a series of assumptions, and considering how far the results are compatible with known facts and conditions. This process involves the dismissal of certain hypotheses, but although he eventually finds one which fulfils the requisite conditions, it does not follow that no other equally satisfactory hypothesis can be found. Consequently his results though interesting cannot be regarded as final. The suppositions he is obliged to introduce before obtaining satisfactory results are, that the density of the subtratum beneath the continental and the sub-oceanic portions of the crust is different, and that the sub-oceanic crust consists of two layers of different densities.

It is conceivable, however, that the lower part of the crust is *everywhere* denser than the upper part, and consequently that two layers of continental crust should be introduced into the problem; whether this hypothesis would likewise fulfil the conditions, and whether it would lead to the same results as that which Mr. Fisher adopts, could only be ascertained by trial. Mr. Fisher informs me that he has not made this trial, and that every additional assumption introduced increases the great labour of the calculations.

Let us assume, however, that no other hypothesis would satisfy the conditions so well as that which he has adopted, and let us see to what conclusions it leads. Mr. Fisher derives from it the following important results:—

(1) That the sub-oceanic crust dips more deeply into the substratum than the continental crust.

(2) That its lower part is more dense than the substratum.

(3) That the density of the liquid substratum is less beneath the oceans than beneath the continents.

This last result leads to the conclusion that the differences of density in the substratum must give rise to ascending and descending convection currents, and that the ascending currents will rise beneath the oceans while the descending currents will occur beneath the continents. "That the former occupy so much larger an area is," he says, "no more than we might expect, because to whatever immediate cause they may be due, they are ultimately the result of secular cooling. . . . The descending being merely return currents will be confined to the smaller area, but on that account they will move the more rapidly."

Finally he says that these conclusions confirm the theory of the permanence of oceans, "because it is difficult to conceive how the subjacent crust, once more dense, can have subsequently passed into the less dense condition which would be requisite to render it continental." I venture to think he is hardly justified in making this unqualified statement, and purpose to show that his results only confirm the theory of the permanence of oceans in a limited and partial manner.

In the first place, if chapters xvii. and xxiv. are read carefully, it will be obvious that Mr. Fisher uses the terms oceanic and sub-oceanic in a special sense. On p. 233 he classes areas having less than two vertical miles of water as "extensions of the elevations that produced the continents," and even those with depths of two to three miles of water he regards as "sometimes connected with and prolongations of the first." In other words, he looks upon the shallower parts of the great oceans from a continental coastline to a depth of at least 2000 fathoms as extensions of the continental elevations.

Again, on p. 331 we find him saying that New Caledonia and the Seychelles are not properly speaking oceanic islands, because the first is a prolongation of the submerged ridge which connects New Zealand with North Australia, and because the latter belongs to an extension of the Madagascar ridge into the Indian Ocean. Now a reference to the physical chart of the oceans given in the "Narrative of the Cruise of the *Challenger*" (vol. i.) shows that the 1000-fathom line completely encircles New Caledonia and the adjacent islands, and that the submerged ridge which he speaks of would be a very narrow one unless we regard it as extending to the line of 2000 fathoms; but this line includes also the Solomon Islands, the Fijis, and the Friendly Islands, so that if New Caledonia cannot be considered as an oceanic island neither can the other islands just mentioned, though no one would reject them from that category on other grounds. Similarly, the Seychelles and Amirantes are surrounded by water of more than 1000 fathoms, and are usually regarded as oceanic islands. The same may be said of Barbados, where stratified Neozoic rocks are found.

The contour-line of 1000 fathoms has, I think, been generally taken by recent writers as the approximate limit of the continental elevations, the space outside this being regarded as oceanic; the islands which rise from depths of over 1000 fathoms would on this view be necessarily classed as oceanic, and as a matter of fact all such islands come within the terms of Sir A. Wallace's definition of an oceanic island except that a few of them are not entirely of volcanic or coralline composition. To exclude all the islands which rise from within the 2000-fathom limit would necessitate the division of oceanic islands into two classes, the definition of which would be difficult.

I am not saying that such a distinction would be incorrect, or that Mr. Fisher has no right to assign larger limits to the continental elevations and narrower limits to the oceans: I only desire to show that he takes a special view, and that he declines to regard islands which rise from less than 2000 fathoms as specimens of the sub-oceanic crust. His discussion of the probable structure of the *sub-oceanic* crust deals therefore with areas which are covered by water of three miles or more in depth—that is to say, from about 2500 to 5000 fathoms, and the comparison which he makes between patches of sub-continental and sub-oceanic crust is really between a piece of continental land and a piece below an area of deep ocean at a considerable distance from the continents.

With regard to this point, I have had the advantage of a further explanation from Mr. Fisher; writing to me he says:— "My sub-oceanic patch may be anywhere under the ocean, but you must remember that all the quantities are subject to change except c, ρ, μ, σ, as δ diminishes; *i.e.* as the ocean grows shallower toward the coast-lines, the thicknesses and densities merge into those at the sea-level, the second layer of the sub-oceanic crust at the same time thinning away to nothing. You are quite right in thinking that in a general way in discussing the sub-oceanic crust I am dealing with the crust at a considerable distance from the continents. . . . . I do not profess to explain the structure of the crust of the earth in those parts which appear to have sometimes been land and sometimes sea. I should, however, guess that having been at times land the crust there resembles the present continental crust. Still the equations (p. 242) must apply to these parts if only we knew what assumptions to make."

Since, therefore, there are regions of sub-oceanic crust the structure of which may resemble that of the continental crust rather than that beneath the central parts of the oceans, it is clearly of importance to consider the position and extent of these regions. Let us first take that part of the Pacific Ocean in which New Caledonia is situate; if we are to regard it as a submerged plateau which may once have been continental land, it acquires a special interest. The contour of 2000 fathoms which unites New Caledonia to Australia and New Zealand extends from the north coast of New Guinea by the Solomon Islands to Samoa, and then bends southward to New Zealand, but curves out again

so as to include the Chatham and Antipodes Islands, some 600 miles to the south-east of New Zealand. Southward it has a connection with the Antarctic continent, but a deep gulf of over 2000 fathoms runs far up outside the east coast of Australia. The area within the 2000-fathom line measures about 2500 miles across its northern portion, and has an extreme length of about 3600 miles from its northern border to the south end of New Zealand.

If this large area is not to be regarded as strictly oceanic—that is to say, if the physical structure of the crust beneath it differs from that of the crust beneath the deeper ocean outside it—and if its geological history is different from that of this deeper oceanic area, and is comparable with that of a continent, then a very important modification is introduced into the theory of the permanence of oceans and continents.

We learn that an area now covered with oceanic deposits may not have been always ocean, and this is precisely what Lyell and his followers have always maintained; for if so large a part of the Pacific may have been land (say in the Cretaceous period), there has been what most geologists would consider to be a change from continental to oceanic conditions; and if, being such a transmutable region, it may eventually be raised again till large parts of it become land surfaces, round which shallow water deposits could be formed, it would exhibit strata of deep-sea origin (usually called oceanic) intercalated between formations of the ordinary continental type.

Another region where similar transmutations appear to have taken place is that of the West Indian Islands with the adjoining area of the Caribbean Sea and a portion of the Western Atlantic. Of this region the structure of Barbados is an illustration. That island conforms to the ordinary definition of an oceanic island; it is separated from South America and the rest of the Antilles by water of over 1000 fathoms, and the scanty fauna which it possesses is not such as would have been introduced by any former land connection. Its geological structure is simple but striking: there are no volcanic rocks, but a basal series of sandstones and clays that are similar to the older Tertiaries of Trinidad, and may be regarded as testifying to a former northern extension of the South American continent; above these are oceanic deposits, consolidated radiolarian and foraminiferal oozes, which appear to be of very late Tertiary age (Pliocene or Pleistocene). Capping the whole are raised coral reefs. Here, therefore, is part of a continental (or shallow sea) area which has sunk into oceanic depths during the Tertiary period, has received a burden of oceanic deposits, and has risen again to be invested with a formation of essentially shallow water origin. Certainly geologists have no proof of greater geographical changes than this, though Europe affords evidence of quite as great a change, for in the area of the European chalk we have an instance of similar oceanic conditions to those under which the Barbados earths were deposited; yet this area was continental land before the Cretaceous period, and has again become so since that period.

The other oceanic areas which have less than 2000 fathoms of water over them are the Arctic Ocean, the southern part of the Indian Ocean, and part of the North Pacific between America and Kamchatka. It would appear then that we may claim these regions, together with the Caribbean area and a large part of the Western Pacific, as areas which have been interchangeable with the present continental surfaces.[1]

Mr. Fisher does not discuss the subterranean structure of the shallow ocean areas, but in his letter already quoted he inclines to think that the crust beneath them is similar to the continental crust, and this view is borne out by the structure of certain oceanic islands; but though the density and general structure of the crust may be similar to that of the continents, the condition of the liquid substratum may not be exactly the same, or rather there may be differences in the force and direction of the convection currents which traverse the substratum.

In chapter xxiv. Mr. Fisher does briefly consider the condition of the substratum in the tracts that lie between the continents and the [deep] oceanic regions. Having shown that, if the density of the substratum is less beneath the ocean than beneath the land, the convection currents must rise beneath the oceans and descend beneath the continents, he points out that there must be a certain space between the lines of ascent and descent where the currents will move more or less horizontally. In this horizontal movement he finds a force capable of exerting strong pressure on the continental crust. Now in some parts of the world the space along which these horizontal currents move may be narrow, but in others it is probably broad: thus, on the east side of the Pacific, where the change from ocean depths to mountain heights is rapid, this space is doubtless small, but on the west side of the same ocean, as we have seen, there is a broad intervening area of shallow ocean, and beneath this the currents that move westward may continue to be mainly horizontal till they reach Australia.

[1] The ridges in the Central and Southern Atlantic do not come within the category of shallow oceans.

The behaviour of convection currents is so little understood that one cannot predicate much about them; there would probably be a certain play of ascending and descending currents beneath the broad semi-oceanic area as well as horizontal currents, and very slight changes may cause these to vary in volume and to alter their positions; such a region is therefore likely to be in a state of unstable equilibrium, and its upheaval or further subsidence would depend on the balance that is established between the three sets of currents in the liquid substratum beneath it.

Another question suggests itself—namely, whether the oceans have always been as deep as they are now. According to Mr. Fisher's results, the mass of the sub-oceanic crust is greater than that of the sub-continental crust, but he gives reasons for thinking that its thickness is not greater, and if this is so, then its density must be greater; and it is from this he deduces the permanency of the oceans, because it is difficult to conceive of the denser crust becoming less dense, which would be necessary before any part of it could be converted into a continent. But though this difficulty certainly exists, it does not preclude the possibility of the sub-oceanic crust having been originally less dense than it is now; it may have been growing denser, and there may have been a corresponding increase in the size and depth of the oceans at the expense of the continents. His results, in fact, do not involve the permanency of the present continents, or of the present relative proportions of land and water surfaces. We are at liberty to imagine a time when there was much more land than there is at present, and when all the oceans were comparatively shallow; there being at this early period less difference in the comparative density of the sub-oceanic and sub-continental crust.

We may, in fact, postulate a secular increase in the size of the oceans and in the depth of the ocean basins corresponding to a secular increase in the density of the sub-oceanic crust; and possibly as a consequence a general increased stability of the whole crust.

The supposition of a secular increase in the depth of the oceans is in accordance with the evidence of geological history, for if there had been such an increase we should expect to find that oceanic deposits of the modern type were essentially Neozoic formations, and would not occur among Palæozoic rocks; and such appears to be the case. At present we do not know of the existence of any purely oceanic limestone that is older than the Cretaceous period; and among the Palæozoic rocks there are none which appear to have been formed at any great distance from continental land.

I think it has now been shown that Mr. Fisher's conclusions do not give unqualified support to the theory of the permanence of oceans, but that, on the contrary, they are consistent with two important limitations of the theory—limitations which had already been suggested by geologists before the publication of Mr. Fisher's book. Thus, Prof. Prestwich has expressed the opinion[1] "that it is only the deeper parts of the great ocean-troughs that can claim the high antiquity which is now advocated for them by many eminent American and English geologists"; and I have suggested the probability that "the tendency of all recent geographical changes has been to deepen the ocean-basins, and to raise the mountain-peaks to higher and higher elevations."[2]

It is therefore satisfactory to find that the results of purely physical and mathematical reasoning, on the one hand, and of a consideration of the geological evidence, on the other hand, are so closely in accord. The importance of this agreement consists in the way it opens for the reconciliation of two opposing geological schools: an important limitation is imposed on the Lyellian belief in the past interchange of oceanic and continental areas; while the extreme view, held by Dana and others, that there has been no such interchange at all, may be equally far from the truth; the probability being that truth lies midway between the two extremes.

It is also worthy of note that the hypothesis of a secular increase in the depth of the oceans and the heights of the moun-

[1] "Geology," vol. ii. p. 547.
[2] "The Building of the British Isles," p. 334.

tains brings the whole succession of past geological changes within the scope of a general theory of geographical evolution.

A. J. JUKES-BROWNE.

The Composition of the Chemical Elements.

MY excuse for troubling your readers with this well-worn theme is that a definite hypothesis is possible, which, should it be fully borne out by the facts, appears to afford a remarkably complete explanation of the periodic law, as set forth in Prof. Mendeleeff's table.

The periodicity exhibited by this table is double, alternate series presenting members which have high or low atomic volumes, are fusible or infusible, &c.

Should the elements be really simple atoms, it would be impossible to account for this fact without introducing occult differences of quality, from which it has been all along the aim of chemical science to free itself. Undoubtedly periodical variations in the size and shape of the atoms might account for the dual periodicity of their properties, but nothing satisfactory can be gleaned from such an explanation. Besides, we are accustomed to regard differences of properties in compounds as dependent on composition, even should their molecular weights be similar. It may also be urged that, if the elements are supposed single, their properties should vary with increase of weight in some continuous manner, and not sway to and fro so remarkably. I am aware that Prof. Mendeleeff himself does not take this view (cf. Chem. Soc. Journ., October 1889), but it is one that is widely spread, and is held by other eminent chemists.

It is, however, possible to push too far such analogies as that of a series of organic compounds. Important differences exist between such a series and that of a natural family of the elements: for example, the specific refraction equivalents are not at all analogous in the two cases. Specific heat determinations show that, as a rule, an element moves as a single solid mass. But these considerations need prove nothing more than that we must be prepared to deal, in the case of the elements, with affinities of a different order—perhaps brought into play by vastly different conditions—from those found in ordinary compounds.

If the elements are assumed to be composite radicles, then, in stating their hypothetical composition, there is material ready to hand. The famous principle known as "Occam's razor" applies here as elsewhere. Hypothetical elements should only be introduced where other considerations are plainly in favour of the suppositions involved.

The elements form natural families of two groups each, six of them having for their types the following: Li, Be, B, C, N, and O.

Since the properties of the typical element run all through the members of a family, then (on the hypothesis that properties depend upon composition) we should expect it to be found in the formulæ of the remainder.

The hypothesis here advanced is, that the periodicity of the properties of the elements is due to the dependence of the properties of each element upon those of the typical element of the family to which it belongs, together with the mode of its combination with oxygen. In other words, that the elements, with the exception of the first six, are, in a qualified sense, compound oxygen radicles.

The reasons for the adoption of oxygen are: (1) the remarkable coincidence of the figures for each family upon this hypothesis; (2) that the atomic weights of the oxygen family of elements are whole multiples of that of oxygen; (3) the relations disclosed between the numbers of atoms composing the elements, which cannot be other than the result of law; and (4) the fact that all the elements combine with oxygen, which is also the most plentiful element in Nature.

Supposing any natural family complete, its two groups are given by the following formulæ, R being its typical element:—

$$R \begin{cases} \text{Group } (a): RO_2,\ RO_5,\ RO_8,\ RO_{11},\ RO_{14}. \\ \text{Group } (b): RO,\ R_2O_3,\ R_2O_6,\ R_2O_9,\ R_2O_{12}. \end{cases}$$

The seventh and eighth families are very incomplete, but may be represented in the same way.

It will be noted that the numbers of atoms in these formulæ are as follow:—

$$1 \begin{cases} 3,\ 6,\ 9,\ 12,\ 15. \\ 2,\ 5,\ 8,\ 11,\ 14. \end{cases}$$

The common difference in each group being 3, and the numbers 4, 7, 10, and 13 being absent.

The resemblance of these figures to the atomic weights of the ten typical elements (including four hypothetical ones) is very close. One is almost tempted to regard them as the primitive forms of the combination of matter, and to return to Prout's hypothesis.

The existence of *four* elements between H and Li is indicated as well by the gap which exists between them as by this hypothesis. That Fe, Co, Ni, &c., have formulæ commencing with R_2, is shown by the fact that they recur regularly in the series having these formulæ, their comparative infusibility and low atomic volume indicating also this composition, as well as the fact that, if it were otherwise, the rule observable in the first six families would be broken through. It is, again, hardly possible to suppose that the seventh family, the halogens, should contain the electropositive hydrogen, although the latter would then lose its unique position, and in this case the *difference* between the calculated values of Ag and I (18·9) agrees very nearly with that between those observed (18·87), the *ratio* of these latter being very exactly determined by Stas. This, however, is a matter which may well be left undecided for the present. Should fluorine be a fundamental element, the halogen series will break the rule which holds for at least six out of the remaining seven families.

The following table is constructed on the lines of Mendeleeff's. The seventh and eighth families are placed first in order, and the calculated and observed atomic weights are placed underneath their respective formulæ. Want of data is indicated by blanks, but the rarer metals are omitted, although they mostly correspond to the formulæ R_2O_9. It will be noted that the arrangement gives Mn, Fe, Co, Ni, and Cu an intelligible position in the series.

It is not to be expected that the calculated and observed figures will perfectly agree, although in some thirty cases the average variation is 0·5 of a unit. The chief variations occur in two series, in which, however, the natural order is preserved, viz. Ti, V, and Cr, with an average error of 4·5, and all the elements containing O_{12}, from tungsten to bismuth, in which the mean difference is 9. It will be noted that this difference holds even in the case of the eighth family, in which the formulæ contain the hypothetical R^{ii}, R^{iii}, and R^{iv}, showing that the errors arise from a common cause. The atomic weights, since the discovery of the periodic law, have not been decided upon without reference to one another. This whole series is separated by a huge gap from the rest of the atomic weights, which is only filled in at intervals by the less common metals of the earths, &c., and consequently an error in one of them would certainly affect the whole. Similarly, the differences of 4 between the observed atomic weights of Ca and Sc, and Sc and Ti, are anomalous.

On the other hand, the coincidences exhibited by the table cannot be the work of chance, and, considering the inexactitude of the determinations of many of the atomic weights, the fact that the average of the differences between the observed and calculated numbers in the large majority of the elements is only one unit, and that the remainder appear to arise from a single cause, is remarkable, especially when we consider the facts which are brought to light by this mode of representation. The law that elements essentially similar differ only by an atomic weight of O_3, or its multiple, surely deserves attention. When, again, the difference between the two groups of any natural family, and the periodicity of the properties of the elements, are exhibited as the result of composition, the conclusion becomes apparent that we have in the hypothesis at least a guide for future research.

The atomic volumes of the groups commencing with RO are smaller than in those commencing with RO_2. These correspond to the "even" and "odd" series of Mendeleeff. Other properties follow, thus affording a possible clue as to *how* the characteristics of the elements depend upon their composition.

Without trespassing further upon your valuable space, I will conclude by quoting Dr. Gladstone (Pres. Address, Chemical Section, Brit. Assoc., Southport, 1883):—

"The remarkable relations between the atomic weights of the elements and many peculiarities of their grouping, force upon us the conviction that they are not separate bodies created without reference to one another, but that they have been originally fashioned, or built up from one another, upon some general plan. This plan we may hope to understand better; but if we are ever to transform one of these supposed elements into another, or to split up one of them into two or three dissimilar forms of matter, it will probably be by the application of some method of analysis hitherto unknown."

VII.	VIII.			I.	II.	III.	IV.	V.	VI.
H 1 R^{i} 3 $F = R^{i}O$ 19 19	R^{ii} 4 — $R^{ii}O$ — —	R^{iii} 5 — $R^{iii}O$ — —	R^{iv} 6 — $R^{iv}O$ — —	Li 7 $Na = LiO$ 23 23	Be 9 $Mg = BeO$ 24 25	B 11 $Al = BO$ 27 27	C 12 $Si = CO$ 28 28	N 14 $P = NO$ 31 30	O 16 $S = O_2$ 32 32
$Cl = R^{i}O_2$ 35·5 35	$R^{ii}O_2$ —	$R^{iii}O_2$ —	$R^{iii}O_2$ —	$K = LiO_2$ 39 39	$Ca = BeO_2$ 40 41	$Sc = BO_2$ 44 43	$Ti = CO_2$ 48 44	$V = NO_2$ 51 46	$Cr = O_3$ 52·5 48
$Mn = R^{i}_2O_3$ 55 54	$Fe = R^{ii}_2O_3$ 56 56	$Co = R^{iii}_2O_3$ 58·5 58	$Ni = R^{iv}_2O_3$ 58·5 60	$Cu = Li_2O_3$ 63 62	$Zn = Be_2O_3$ 65 66	$Ga = B_2O_3$ 70 70	$Ge = C_2O_3$ 72 72	$As = N_2O_3$ 75 76	$Se = O_5$ 79 80
$Br = R^{i}O_5$ 80 83	$R^{ii}O_5$ —	$R^{iii}O_5$ —	$R^{iv}O_5$ —	$Rb = LiO_5$ 85 87	$Sr = BeO_5$ 87 89	$Y = BO_5$ 90 91	$Zr = CO_5$ 90·5 92	$Nb = NO_5$ 94 94	$Mo = O_6$ 96 96
— $R^{i}_2O_6$ — 102	$Ru = R^{ii}_2O_6$ 103·5 104	$Rh = R^{iii}_2O_6$ 104 106	$Pd = R^{iv}_2O_6$ 106 108	$Ag = Li_2O_6$ 108 110	$Cd = Be_2O_6$ 112 114	$In = B_2O_6$ 113·5 118	$Sn = C_2O_6$ 118 120	$Sb = N_2O_6$ 120 124	$Te = O_8$ 126·5 128
$I = R^{i}O_8$ 126·5 131	$R^{ii}O_8$ —	$R^{iii}O_8$ —	$R^{iv}O_8$ —	$Cs = LiO_8$ 133 135	$Ba = BeO_8$ 137 137	$La = BO_8$ 139 139	$Ce = CO_8$ 141 140	$Di = NO_8$ 145 142	— O_9 — 144
— $R^{i}_2O_9$ — 150	— $R^{ii}_2O_9$ — 152	— $R^{iii}_2O_9$ — 154	— $R^{iv}_2O_9$ — 156	— Li_2O_9 — 158	— Be_2O_9 — 162	$Er = B_2O_9$ 166 166	— C_2O_9 — 168	— N_2O_9 — 172	— O_{11} — 176
— $R^{i}O_{11}$ — 179	$R^{ii}O_{11}$ —	$R^{iii}O_{11}$ —	$R^{iv}O_{11}$ —	— LiO_{11} — 183	— BeO_{11} — 185	— BO_{11} — 187	— CO_{11} — 188	— NO_{11} — 190	$W = O_{12}$ 184 192
— $R^{i}_2O_{12}$ — 198	$Ir = R^{ii}_2O_{12}$ 192·5 200	$Pt = R^{iii}_2O_{12}$ 194·5 202	$Os = R^{iv}_2O_{12}$ 195 204	$Au = Li_2O_{12}$ 196 206	$Hg = Be_2O_{12}$ 200 210	$Tl = B_2O_{12}$ 204 214	$Pb = C_2O_{12}$ 206·5 216	$Bi = N_2O_{12}$ 210 220	— O_{14} — 224
— $R^{i}O_{14}$ — 227	$R^{iii}O_{14}$ —	$R^{iii}O_{14}$ —	$R^{iv}O_{14}$ —	— LiO_{14} — 231	— BeO_{14} — 233	— BO_{14} — 235	$Th = CO_{14}$ 232 236	— NO_{14} — 238	$Ur = O_{15}$ 240 240

Mirion Terrace, Crewe, October 25.

A. M. STAPLEY.

Is Greenland our Arctic Ice Cap?

THE result of Dr. Nansen's journey across Greenland, establishing, as it practically does, that this Arctic continent is covered by a huge ice cap, promises to be a matter of some interest in several ways.

Among other things it may possibly yield a clue as to the cause of the south polar cap of Mars being so very excentrically placed.

Since the time of the elder Herschel this has been a subject of speculation, and various ingenious suggestions have been put forward by astronomers to account for the presumed anomaly.

Webb, in his "Celestial Objects," p. 147, tells us that Herschel found that the caps were not opposite each other; and says himself that "one would expect that they might have been diametrically opposite."

"Mädler and Secchi found the north zone concentric with the axis, but the south considerably excentric"; and "it has been suggested by Beer and Mädler that the poles of cold may not coincide with the poles of rotation."

Later on, at p. 148, he tells us that "Secchi found the appearances at the poles irreconcilable with the idea of circular caps, and was forced to adopt the supposition of complicated and lobate forms. Schiaparelli alludes to the possibility of a mass of *floating ice*."

Apparently it was taken for granted that the *ice* or snow caps of Mars, should not only be truly circular in form, but centrally placed over the axis of rotation, like the cloud caps of Jupiter and Saturn.

But it seems to me that Dr. Nansen's journey will go a long way towards solving this problem, by demonstrating that Greenland is practically one of our two polar ice caps. On our South Pole we have one, more or less centrally placed over the axis of rotation, and which certainly does not float about, having two large active volcanoes on it. It corresponds fairly well to the northern pole of Mars. But on our North Pole—as far as we can see—there is no large permanent ice cap, and in its place we have an irregular, extensive polar basin.

Roughly speaking, we may say that the character of the Arctic and Antarctic ice bears this out, for in the south we see the immense flat-topped bergs of 2000 feet thickness, and several miles long, which are obviously portions of the southern ice cap broken adrift. In the north we see a preponderance of floe, or thin field-ice, a few flat-topped bergs near Franz Joseph Land (Young), and the angular bergs of the Atlantic, mainly from West Greenland (Greely).

If our Arctic basin is deep and has few islands in it, it stands to reason that a permanant ice cap could not form, or become anchored, there; the floe would be perpetually broken up by storms and tides, carried away, and melted. A floating ice cap would be impossible. The presence of a polar continent—even excentrically placed—would seem to be necessary, as in the case of Greenland. This would indicate the solution for the supposed anomaly, *re* the position, of the south polar cap of Mars, and for the lobate appearances remarked by Secchi in 1858.

If the foregoing remarks are at all likely to be correct, Dr. Nansen's journey may have quite unexpectedly solved for us an interesting astronomical problem, and thereby afforded another clue to the condition of Mars, a proof almost of partial glaciation.

I believe that M. Fizeau regards the so-called "canals" as evidence of the "movement and rupture" of a glacial crust.

But if this crust is formed on, and attached to, any extensive *land* surface (such as Greenland, say), it is not easy to account for such enormous ruptures, and the lateral movement.

If the canals are looked on as huge lanes of open water in a floating ice-pack, they would vary in size and form almost daily.

Sibsagar, Assam, India, September 25. S. E. PEAL.

Globular and other Forms of Lightning.

MR. A. T. HARE'S account in NATURE, vol. xl. p. 415, of a flash of globular lightning seems to illustrate so well the explanation which I gave, many years ago, of the formation of fire-ball lightning, that the following extract from my pamphlet "On Atmospheric Electricity" (London, Hardwicke, Piccadilly, 1863) and the remarks which I have appended to it, may perhaps not be without interest at the present time. The pamphlet is not now on sale. The quotation is from pp. 45–46; I omit a few references:—

"A slip of tin-foil was formed into a hollow cylinder, and thrust tightly into one end of a glass tube which was about $\frac{7}{16}$ inch in external diameter, and the glass was not very thick. A brass ball was fixed to the end of the glass tube, and the tin-foil extended from the ball to the distance of about $12\frac{1}{2}$ inches from it, and all the tin-foil was inside the glass tube. The remainder of the glass tube served for an insulating support to the part which held the tin-foil. On electrifying the ball, the electricity is conveyed by the tin-foil to the inside surface of the lined part of the glass tube; and at the same moment the outside of this part of the tube is electrified inductively, and with the same sort of electricity as that with which the interior of the tube is charged. The part of the tube which held the tin-foil was supported horizontally. There was also a copper hook which could be set on any part of the outside of the lined portion of the glass tube.

"The copper hook was set at a distance of $7\frac{1}{2}$ inches from the brass ball on the end of the tube, and was connected with the outside of a Leyden-jar which was charged so as to be nearly able to give a spark $\frac{1}{2}$ inch long between two other brass balls each of which was $1\frac{1}{4}$ inch in diameter. The knob of the jar was next brought to the ball on the end of the glass tube; the discharge readily passed over the $7\frac{1}{2}$ inches of the electrified outer surface of the glass tube. Sometimes the spark could pass when the hook was at $8\frac{1}{2}$ inches from the ball. When the hook was placed at a distance of $12\frac{3}{4}$ inches from the ball, the spark passed between the ball and the hook with a much lower charge in the jar than was necessary to produce a spark $\frac{3}{4}$ inch long between the pair of balls before mentioned.

"These experiments show that the length of an ordinary electric spark, can be much increased by causing the spark to pass over an electrified surface. Instances of this are seen in the spontaneous discharge of Leyden-jars, and in the long sparks which flash over the revolving glass of the electrical machine.

"Let a ball be attached to the prime conductor of the electrical machine so that the ball may give electrical brushes to the air. Much longer sparks may be drawn from the ball along the path of the brushes than from the other parts of the prime conductor. The brush discharge electrifies the air in the neighbourhood of the ball, and the spark is longer because it passes near to, or through, a mass of previously charged particles.

"It is well known that atmospheric electricity not unfrequently forms an electric fire-ball which moves but slowly, and which, on striking an object, explodes and produces all the usual effects of a flash of lightning. Sir William Harris writes:—'Now, it is not improbable that, in many cases in which distinct balls of fire of sensible duration have been perceived, the appearance has resulted from the species of brush or glow discharge already described, and which may often precede the main shock.' And Dr. Noad says of the electrical fire-ball that 'it is no doubt always attended by a diffusely-luminous track; this may, however, be completely eclipsed in the mind of the observer by the great concentration and density of the discharge in the points immediately through which it continues to force its way.' A more perfect explanation can, as I suppose, be given by the aid of the experiments of this chapter.

"A thunder-cloud may produce both the electric glow and the electric brush, at the end of one of its cloudy branches. And since electricity passes freely along a charged surface, therefore the glowing discharge by electrifying the air in front of the aërial conductor, adds continually to the length of the conducting column, and so the electrical fire-ball advances. Little drops of water, or any other conductive matter which the column finds in its course, must facilitate the transmission of the electricity to the fire-ball; and without doubt, too, the electricity of the column continues to spread laterally, and so it increases the conductive capacity of the column. The electricicity travels through the electrified column as a series of luminous disruptive discharges; but the light is brightest at the head, because there the diameter of the column is least, and the discharge is most closely packed; and because there the air is unelectrified, and consequently opposes so great resistance to the passage of the electricity. As soon as the fire-ball has arrived at a conducting mass on the earth, the aërial conductor has been completed, and a flash of lightning may instantly follow along the path of the fire-ball."

Since the Leyden-jar, with a charge somewhat less than that required to give a spark $\frac{1}{2}$ inch long between the $1\frac{1}{4}$-inch brass

balls, gave a spark about 8 inches long over the excited glass tube; and since the Leyden-jar, with a charge much lower than that required to produce a spark $\frac{3}{4}$ inch long between the two brass balls, was sufficient to give a spark about 13 inches long over the excited glass tube; it was at once seen that the length of the spark over the excited glass tube, increases faster than the intensity of the charge of the Leyden-jar. Of course the law which connects the length of the spark over the excited glass tube, with the intensity of the charge of the Leyden-jar, can only be determined by experiment. It is, however, to be noticed that, from the experiments of Harris and others, the length of a spark in air of a Leyden-jar varies directly with the intensity of the charge—that is, with the quantity of electricity in the jar as measured by any such contrivance as the unit-jar. And further, that the length of the spark over the excited glass tube depends (1) on the length of the spark which the charge of the Leyden-jar can produce between the $1\frac{1}{4}$-inch brass balls; and also (2) on the degree of electrification of the glass tube; and that both these two quantities—namely, (1) and (2)—increase together. From these considerations, I should expect to find that the length of the spark over the excited glass tube increases in some way with the square of the intensity of the charge of the Leyden-jar—that is, with the square of the potential.

I dare say that the sparks over the excited glass tube, would become very brilliant by using an induction coil to charge the Leyden-jar. But to produce the maximum effect, the glass tube should, I think, be lined, as in the following experiment, with tin-filings instead of the tin-foil.

A piece of hard German glass tube was taken, and one end closed at the blow-pipe and the other end bordered to receive a cork. After these operations, the tube was found to be just 2 feet $2\frac{5}{8}$ inches long; the external diameter of the tube was $\frac{11}{16}$ inch, and the glass was $\frac{1}{20}$ inch thick. Next, the closed end of the tube was filled with tin-filings to the height of 6 inches, the filings having been condensed by tapping the end of the tube on a piece of wood. A brass rod, with a knob at one end and a screw having been cut on the other end, was screwed into a cork which nicely fitted into the glass tube, and, by means of the rod, the cork was thrust into the tube until it pressed upon the tin-filings, and since the point of the rod was sharp and projected beyond the cork, the end of the rod entered a little way into the tin-filings. The knob of the brass rod now stood just at the mouth of the glass tube, and the mouth of the tube also contained a cork through which the brass rod passed. Of the outside of the glass tube, the part surrounding the tin-filings was painted over with lac varnish, and, as soon as it became sufficiently sticky, a thin piece of tin-foil was wrapped around the tube so as to cover the tin-filings, and no more. Lastly, the remaining portion of the outside of the glass tube was painted over with lac varnish. To charge this tubular Leyden-jar, it was laid with the tinned end on one conductor and with the knob of the brass rod on the other conductor of a Wimshurst influence machine. I may mention, in passing, that the capacity of this tubular Leyden-jar was surprisingly great in comparison with its size; thus showing that Leyden batteries, both cheap and compact, can be made with the aid of glass tube and metallic filings. The capacity is no doubt due, more or less, to the uniform thinness of the glass, and to the close contact of the tin-filings and the glass. The specific inductive capacity of hard German glass does not seem to have been ascertained. But of course, for the construction of Leyden-jars, and also for the plates of the Wimshurst machine, glass of the highest available specific inductive capacity should be used. It may not be amiss to remark that, owing to the high specific inductive capacity of glass as compared with air, the efficiency of a Wimshurst machine is probably much more increased by diminishing the thickness of the stratum of air between the glass plates than by diminishing the thickness of the plates.

Now, the Leyden-tube produces a class of sparks which I do not think have been shown by any other Leyden-jar. The Leyden-tube was laid, as before mentioned, on the two conductors of a Wimshurst influence machine, and the discharging balls belonging to the conductors were set $\frac{1}{2}$ inch apart. These two discharging balls were each $1\frac{3}{4}$ inch in diameter. On turning the handle of the machine, the Leyden-tube continued, of course, to become charged and then to be discharged by the $\frac{1}{2}$-inch spark between the discharging balls. But besides the main spark between the discharging balls, little streams of electricity appeared along the glass tube, and extended away from the tin-foil to a distance of $1\frac{1}{2}$ inch or more. These sparks were, I think, best seen in a subdued daylight. They were very numerous with each discharge of the tube; I estimated the number of sparks in different discharges as varying between one and two dozens. The sparks were sinuous, very bright at the tin-foil, and tapering away to nothing at the further end. Some of the sparks, however, were not so bright as the others, and rather ruddy; they were probably inside the glass tube, and coloured by the varnish on the tube.

In the *Leisure Hour*, November 1888, p. 777 (56 Paternoster Row), there is a photographic picture of a lightning-blaze, wherein the bright ends of several of the flashes are seen to be sitting upon what appears to be rock, and the flashes bear a strong resemblance to the little sparks whose bright bases rest upon the edge of the tin-foil.

In the *Leisure Hour*, November 1886, p. 786, there is another representation of a flash of lightning from a photograph. In this instance, the flash is thick in the middle, but on approaching the earth, it tapers off to a fine point. Like as a river may be only a small stream at its source and by gathering water as it leads on to the sea, become a bulky stream at its mouth; so the sparks on the Leyden-tube gather up electricity from the Leyden-tube, and so brighten away to the tin-foil. But in this flash of lightning, the very reverse appears to take place. The flash is greatly weakened before it reaches the earth, through a transverse discharge to the air. For around the brighter portions of the flash, the air is shining, and streamers are darting earthwards from the flash into the air. At the upper part of the flash, there are also streamers acting manifestly as feeders from the cloud to the flash. The flash rather resembles a long spark from the prime conductor of an electric machine, than the spark of a Leyden-jar; but the prime conductor being metallic, can only imperfectly represent the much lower conduction of a cloud.

In the *Leisure Hour*, September 1889, p. 641, there is an engraving from a photograph of the so-called ribbon-lightning. This form of lightning is clearly produced by a succession of flashes following along the same path, combined with some slight motion given to the camera by the hand of the operator; as indeed is there pointed out. The question is, How comes it that the flash so repeatedly passes along the same path? The answer there given is that suggested by Mr. Cowper Ranyard, "That apparently the first flash would heat the air and slightly rarefy it, leaving a path of least resistance, along which subsequent discharges would flow as certainly as water follows the twists and turns of a pipe." It seems to me, however, that a far more important cause for making a second flash to pass along the path of its predecessor is to be found in the action of the transverse discharge, whereby a tubular mass of air becomes electrified around the path of the first flash; and through the electrified air, the flash readily passes, as previously shown. In the woodcut, the effulgence of the surrounding air and the streamers show that the lightning was distributing electricity along its path. The transverse discharge is perhaps never absent from the flash of lightning. In NATURE, vol. xl. p. 543, a flash of lightning which struck a windmill, is described as "a mass or network of flame, which threw off thousands of sparks like fireworks."

The discharging balls of the Wimshurst machine were set one inch apart, everything else remaining as before. The sparks now extended along the glass tube to a distance of about $3\frac{1}{2}$ inches from the tin-foil. The general character of the sparks was the same as before, when the discharging balls were set half an inch apart.

The discharging balls were set $1\frac{5}{8}$ inch apart. When the discharge occurred, the sparks extended along the tube to about $5\frac{1}{2}$ inches from the tin-foil. The sparks were straighter, and not nearly so numerous as when the discharging balls were set at half an inch; they were also very much brighter, but like the others, they all tapered away to nothing. In this experiment, the Leyden-tube was charged to about the highest potential that the machine would give it; and the matter was not any further pursued.

REUBEN PHILLIPS.

1 Bay View Terrace, Northam, Bideford, October 9.

"Darwinism."

WHAT my "laborious essay" "distinctly professes to be" is, as its title-page announces, "an *additional* suggestion on the origin of species"; and this additional suggestion is forthwith stated to be that of "*another factor* in the formation of species, which,

although quite independent of natural selection, is in *no way opposed* to natural selection, and may therefore be regarded as a factor *supplementary* to natural selection." This passage occurs in the most conspicuous part of the paper, viz. at the close of the introduction. In the next most conspicuous part—viz., at the close of the paper itself—it is said, "Without natural selection, physiological selection would be powerless to create any differences of specific type, other than those of mutual sterility, and trivial details of structure, form, and colour."

So much for distinct professions. But as I am tired of controverting the statement that I both intended and perpetrated an "attack" on Mr. Darwin's theory, I will not now burden your columns by supplying the context, or otherwise easily explaining the passages which Prof. Lankester quotes in support of this statement. On a future occasion, however, I hope to avail myself of a more fitting opportunity fully to display the relation in which my "laborious essay" stands to the work of Mr. Darwin; and then I trust it will be clearly seen that, whatever we may severally think about the "complementary principle" of physiological selection, at all events it is in no way hostile to the cardinal principle of natural selection.

Edinburgh, November 19. GEORGE J. ROMANES.

How not to Teach Geometry.

As I have come across an almost unforeseen development of the above heading, I take the liberty of bringing it before your readers. For myself, I may state that I have considered the "learn a proposition off by heart" method was sufficiently bad, but what is to be made of the method described in the following extract from a note which I recently received from my friend:—

"We have half of a proposition written on the board, and then we write it at home from memory; then the other half is written on the board, and we write that at home from memory. Then we have to learn the whole proposition at once, to be able to write or say it with different letters. We are not allowed to have a printed Euclid book—we are only allowed to have a book of Enunciations."

Of course this refers to Euc. i. 1.

I beg to commend the above extract to the Association for the Improvement of Geometrical Teaching. I do not know whether to add the name of the school where the above system is followed by one of the teachers.

HERBERT J. WOODALL.

Normal School of Science, South Kensington, November 11.

P.S.—I should like to see opinions on the teaching described.

A Brilliant Meteor.

Is not the meteor seen from Warwick School on November 4 the same as that mentioned in the following from my daughter, written from the school at Brookfield, Wigton, Cumberland?

"On Monday night (November 4), at 7.55 p.m., when out on the playground viewing the stars, I saw a most beautiful meteor. It seemed to be very near, and was in sight for quite a long time. It appeared just over Skiddaw—that is to say, due south—and went towards the south-east. It had a long tail of light, and burst, and sent out beautiful colours, and disappeared near the horizon."

I may add that, last Sunday, November 10, at about 5.56 p.m., I saw here a very bright meteor pass from a point perhaps south-south-west, and altitude about 25°, to a point perhaps south by east, and altitude about 10° or 12°. It was brighter than Venus when the planet is at its brightest, I think; and it seemed to flash out still more brightly just before disappearing; but the colour did not change perceptibly from its former soft white light, and there was no appearance of bursting. At the time of disappearance, its train of light must have extended over several degrees.

WM. SCARNELL LEAN.

Ackworth, November 16.

THE CAUSES AND CHARACTER OF HAZE.

UNLIKE fog, haze commonly occurs in this country when the lower air is in a state of unusual dryness. It is not only a frequent accompaniment of a spell of fine dry weather, but may be, when in combination with certain other conditions, a sign of its approach. Night or morning fogs, and in winter persistent fogs, often signify a calm and settled condition of the air and the prevalence of fair weather. Heavy dews, especially in the autumn, likewise portend fine weather, but usually of shorter duration. Fogs appear usually in one of two conditions: either the air is nearly saturated up to a considerable height, or else is unusually dry, except in a stratum immediately above the ground. In the first case, radiation or condensation from some cause produces, by a slight lowering of temperature, a large precipitation of vapour; and in the second case, radiation from the earth's surface being excessive, owing to the diathermancy of the dry atmosphere, the stratum next the ground rapidly reaches its dew-point, fog is formed, and this fog continues to radiate to the clear sky and further to reduce temperature. Haze, on the other hand, appears often in weather distinguished by unusual dryness, on the surface as well as at a considerable altitude above the ground. The air remains for many days uniformly dry, the nights being nearly dewless, and the sky often free from clouds. The chief difference to be observed, then, is this, that fog requires saturation where it occurs, while haze seems to be favoured rather by a dry atmosphere.

Haze does not prevail on the continent of Europe or in the interior of North America to anything like the same extent as in England; nor, probably, in mid-ocean to the same extent as near the shores of northern countries. On the east coast of Scotland, and, indeed, over all North Britain, it is exceedingly common, especially in the spring, and during the prevalence of east wind, although with west winds the atmosphere is frequently clearer in summer than in Southern England. Over Southern England it is a common accompaniment of winds between east-south-east and north-east inclusive. It appears to prevail more on the eastern than on the western coasts when east winds are blowing. In Western Surrey, when the lower air moves from a westerly direction or is calm, the approach of east wind is announced by a light haze obscuring distant views, before the east wind has actually arrived on the spot of observation. This is not in all cases due to the descent of London smoke from a higher stratum, where the east wind first gains ascendancy, for the phenomenon may be observed in other localities. The haze produced on the first arrival of the east wind is thicker than that which remains when the east wind has gained a strong hold, and the neutral band where calm prevails between a south-west and a north-east current is marked by the thickest mist. In winter a dark fog frequently marks this neutral zone, often not more than one or two miles in breadth, and the zone moves eastwards or westwards according as the west or east wind exercises the strongest pressure. I have frequently observed this phenomenon with great distinctness. In winter, the approach of the equatorial after the prevalence of the polar current is often betokened by a damp fog and the contrary change by a dry fog; the same changes in summer are respectively marked by a great increase of transparency and by a spreading haze or mist. The following observations taken in Scotland illustrate the phenomena accompanying a change from west to east in August. St. Fillan's Hill is a small, steep, isolated volcanic cone about 300 feet in height, standing in the middle of the valley of the Earn, about two miles from the lower end of Loch Earn, in Perthshire. The air was clear, and a fresh westerly breeze was blowing when I was on the summit, about 5 p.m. The breeze suddenly began to slacken, and in about five minutes had dropped altogether. Then down the valley eastwards a blue haze began swiftly to climb the glens tributary to Strathearn, and the whole air eastwards grew obscure. The calm only lasted a little more than two minutes, and then suddenly a strong wind from the east set in, and soon the air, westwards as well as eastwards, was robbed of its transparency. The east wind

continued, and in a few minutes the tops of the hills, which rise precipitately from Strathearn to a height of about 2000 feet, were obscured with cloud-banners growing continuously and descending till in about two hours not only the hills above a level of about 1000 feet, but the whole sky, were covered with gray cloud. The duration of the neutral calm, from two to four minutes, seems to be about the usual time occupied by a moderate east wind in driving back the opposing current, according to my observations in the neighbourhood of London. In the suburbs south-west of London such a change is signalized in the neutral band of calm by a dense yellow haze, producing great darkness, the result of a banking up of smoke to some altitude, together with the condensation of aqueous vapour by the mixture of currents differing in temperature. With lighter winds about equal to each other in momentum, such a band often lasts much longer, and I have known a west wind prevail at Richmond simultaneously with an east wind in London, both without fog, while at Wandsworth, between the two, a calm continued for many minutes, with dense, almost nocturnally-black, smoke-fog, the pressure in each direction being apparently equal. Generally speaking, the mist thus produced at the junction of the two winds is exceedingly dense in winter, moderately dense in spring and autumn, and thinnest in summer, varying, in fact, from a black fog in the cold season to a mere haze in the warmest weather. Hence we have an ascertained condition for the production of haze—the mixture of two opposite winds. It may be here remarked that a very sudden squall of wind from the north, displacing an equatorial or south-westerly current, produces a somewhat similar dense wall of mist, which it soon drives away before it.

Haze very frequently prevails during a north-east or east wind in all parts of Great Britain; in the east of Scotland it is, perhaps, more marked than in other l calities, and attends both wet and dry weather. A dense blue mist or haze brought by the east wind sometimes invests the landscape for days before a continuous downpour from that quarter. This haze extends far out to sea eastwards. The southern parts of England are less troubled than the northern by this disagreeable infliction, and the northern parts of France less still. In the eastern counties, and probably in other parts of England, the density of the haze seems to increase in some proportion to the dryness of the air, when only a slight wind blows. On thoroughly rainy days, such as the north-east wind sometimes brings to the London district, the amount of haze is below the average; and when the north-east wind is accompanied by snow-showers, as it often is in February and March, or by rain-showers later in the year, it is remarkably and conspicuously clear. I cannot remember any showery days with a steady north-east wind showing a true haze, beyond the influence of London, but have often observed the extraordinary clearness of such days, and the apparently dissipative action of the air on London smoke.

Generally, the density of the haze is less as the strength of the wind increases. A gale from the north-east is seldom accompanied by much haze inland, although on the east coast the combination is not uncommon. Haze appears to diminish as the north-east wind grows more established, and in winter a long period of this wind may be experienced without the continuance of haze. It is also important to observe that, when high upper clouds are seen to be moving from a direction between east and north inclusive, but especially from north-east, the air is usually clear, and a long continuance of the polar wind may be expected. It is a sign of the firm establishment of the north-east wind when high cirro-cumulus is seen passing over from that direction, whatever deviations may take place temporarily on the earth's surface. The extension of the north-east wind to a great altitude seems to deprive it of its accustomed haziness. When, on the other hand, thick haze accompanies the north-east wind, if upper clouds are in view, they are generally seen to be borne by a different current, and in winter the lower wind does not, in such conditions, often remain long in the same quarter. Hence we have the means of making forecasts with tolerable safety as follows:—

(1) If the lower air be clear, whether clouds at a high level be seen to move from the north-east or none be visible, the lower wind from north-east will probably last some days, perhaps some weeks.

(2) If the lower air be very thick and misty, the north-east wind is not strongly established, and is likely soon to be succeeded either by variable airs and calms, or by breezes from a different quarter.

In spring and summer, haze prevails sometimes for many days together, with a dry atmosphere, over the whole or a large part of Great Britain. The wind is either easterly or variable, the barometer high, temperature high by day and low by night, and the deposition of dew either small or heavy. The haze seems to be uniformly distributed through the atmosphere, and varies neither from one day to another, nor from day to night. The sky is pale blue, the sun rises and sets red and rayless, and the moonlight reveals the blue mist unchanged by the absence of the sun's rays.

Haze has been known to affect a great part of Europe during a period corresponding with the prevalence of drought.

The formation of haze seems to be more common and more sudden in mountainous regions than on the plain. I had once an opportunity of observing the rapid production of a very dense haze from the top of Cader Idris, in Wales. The morning was bright, fine, and clear, but the heat very oppressive. About midday, signs were seen of an approaching thunderstorm, which, however, spent its force at some distance down the valley. Before the storm, a haze quickly gathered, and completely obscured even the nearer ranges. This haze resembled that which prevails sometimes during many hours before the occurrence of a thunderstorm in the level country.

The conditions favourable to the production of haze may be conveniently summed up as follows:—

(1) A gentle wind from east-south-east to north-east inclusive, and east wind in general, especially with dry weather in spring and summer. If the east wind be established up to a great height, the lower air is usually clear, but if the upper current is from a westerly direction, haze prevails.

(2) Fine settled weather, with variable currents, a dry air, and little dew.

(3) Opposition of currents—such as occurs when several shallow barometric depressions exist over the country—and the atmospheric state preceding thunderstorms.

(4) Damp weather, with light winds and varying temperature, as thaw after frost, with snow on the ground.

Turning to those conditions which are most unfavourable to the production of haze, or in which the air is most transparent, we find them to be—

(1) A state of great humidity, such as that which occurs often before bad weather, the wind being between south and west.

(2) Strong winds and showery weather.

(3) Winds between south-west and north.

(4) Fine settled summer weather, with westerly or southerly winds.

(5) Settled easterly or northerly winds, with either clear sky, or high clouds moving from those directions.

(6) Easterly or northerly winds, with a high continuous cloud canopy moving in the same direction, small range of temperature, and steady conditions; or, with detached cumulus in the daytime, and clear nights.

(7) North-west following a wind between north-west and south is particularly clear, except in thundery weather.

It thus appears that the most striking characteristic which may accompany the formation of haze is an unusual dryness of the air, and that a total absence of haze is often observed when the air is unusually charged with vapour. It does not follow that haze, or a light fog much resembling it, is not also seen in a damp state of the air, or that a saturated air is always free from haze; indeed, something much resembling a dry haze does occur with sudden changes of temperature in all ordinary hygrometric states in our climate. But the very condition to which haze in England is commonly, and in a certain sense correctly, attributed—namely, atmospheric humidity—is, if sufficiently uniform and extended, least favourable to its manifestation. A constant moisture-laden westerly breeze would give a climate nearly as clear as that of the south-west corner of France.

Two principal factors go to the production of ordinary haze: the first, a rather large amount of vapour between the earth and a great altitude, say 60,000 feet; and the second, a mixture of two heterogeneous masses of air. Evidence of the correctness of this proposition is to be found in the geographical distribution of haze and the state of the winds when it occurs.

The causes of fog are either radiation of heat from the earth into space and cooling of the overlying humid strata of air to a temperature below the dew-point, or else the mixture of two winds, differing in temperature and other conditions, one of the currents being usually near its point of saturation previous to contact with the other.

If the above-mentioned statement of the causes of haze be correct, we shall be enabled to account for the appearance of haze in certain conditions, which have been given, and for its absence in others. Taking them in order—

(1) A gentle wind from east to north-east inclusive is favourable to haze, especially if it extends to no very great height. Often the approximate depth or height of the easterly current is difficult to ascertain; but, in general, if it be of short duration, it is shallow, and sometimes upper clouds from a westerly direction may be observed. In these cases especially haze prevails. Considering the shallowness of lower winds compared with their extent—an easterly wind, for instance, which has travelled 300 miles beneath a westerly wind only four miles above the earth's surface—it is quite certain that a very large admixture of the two currents must take place. And we may be sure that in the majority of cases the easterly surface wind has above it an upper current from a westerly direction. Mr. William Stevenson (*Edinburgh Philosophical Magazine*, July 1853) observed the cirrus cloud at Dunse, Berwickshire, for eight years, and from his summary of the direction of the motions of that cloud we derive the following figures:—

	Per cent.
Direction of motion of cirri from between south-west and north-west inclusive	75·2
Direction of motion of cirri from between north and east inclusive	10
Other directions	14·8
Direction of wind at surface of the earth from south-west to north-west inclusive	54·6
Direction of wind at surface of the earth from north to east inclusive	32·4
Other directions	13

Thus there remains a difference of over 20 per cent. excess of westerly upper current over westerly surface wind, and at the level of the cirrus a wind between north and east only prevails once to every three occasions of a surface wind from that quarter. The significance of these figures is not seriously affected by the idea, first suggested by Admiral Fitzroy, that visible cirrus is less likely to form in the polar than in the equatorial current, and any careful observer can easily satisfy himself that westerly winds are more common and easterly winds less common at the cirrus level than on the surface. Mr. Buchan ("Handy Book of Meteorology," p. 230) remarks that, as the north-west current advances into southern latitudes, the increasing heat of the sun will tend to dissolve the cirri which mark its course, and he therefore thinks that the north-west upper current is the most prevalent in Great Britain. The actual numbers obtained by Mr. Stevenson during the eight years were 243 for north-west, and 256 for south-west direction of cirrus.

Mr. Ley ("Laws of the Winds," Part I. p. 154) remarks:—"The fact, indeed, that the observed westerly upper currents prevail over the observed easterly upper currents, even more than the westerly surface winds do over the easterly surface winds, has been admitted by most of the observers who have investigated the subject in different parts of Western Europe; and the same phenomenon is noticed in similar latitudes of North America. . . . Be this as it may, the theory of prevalent polar upper currents derives no support from our own collection of examples. Again, the results of the observations classified in Table IV. appear altogether adverse to the supposition that an easterly upper current is common over the northern portions of those depression systems whose westerly winds are the strongest at the earth's surface. . . . Instead of easterly upper currents, we find a great preponderance of southerly currents."

Out of nine balloon ascents recorded in Glaisher's "Travels in the Air," in which the wind at starting from the surface was easterly, there was not one in which a different current was not encountered at a moderate elevation. The changes were as follows:—

Date.	Surface Wind.	Wind at
April 18, 1863.	N.E.	A moderate height, N.
July 11, 1863.	E.	A moderate height, N.
		5400 feet, N.N.W.
May 29, 1866.	N. by E.	Above 2000 feet, N. by W.
		5100 feet, nearly calm.
Mar. 31, 1863.	E., gentle.	Between 10,300 and 15,400 feet, W.
		About 15,400 feet, N.E.
		Higher still, S.W. and W.
Jan. 12, 1864.	S.E.	1300 feet, strong S.W.
		4000 feet, S.
		8000 feet, S.S.W.
April 6, 1864.	S.E.	About 9000 feet, N.W.
June 10, 1867.	Surface calm,	Higher, N.N.E.
	low elevation	Higher still, N.
	N.E.	
Aug. 12, 1868.	N.E.	5000 feet, S.W.
June 16, 1869.	N.E.	10,000 feet, S.W.

On one occasion—January 12, 1864—the temperature from 3000 to 6000 feet was higher than on the surface, but at 11,500 feet it was more than 30° colder—namely, 11°. A large number of balloon ascents show not only a variety of currents, but large and sudden variations of temperature within a few thousand feet.

Thus we may confidently assume, in the majority of cases of east wind, and especially when this wind is of brief duration, local, or gentle, that a westerly wind flows above it at no great distance from the surface of the earth. Considering the perpetual rapid interchanges (hardly to be called diffusion) going on in the atmosphere, the lower wind must be largely mixed with air of a different condition derived from the westerly current. If a cold dry east wind be permeated by patches and filaments, however minute, of moister and warmer air, they must be cooled by contact with the polar wind, and a slight deposition of vapour may take place. Or the countless invisible dust particles may, by increased radiation towards space through a drier air, either cause a slight deposition of moisture upon themselves or collect still smaller particles together, as dust is known to collect on cold surfaces in a warm air. If deposition of moisture take place, the dryness of the air prevents the water particles from growing to anything like the size of the

particles of a fog; a relatively small diffused quantity of vaporous air in minute parcels could not produce by condensation any but extremely small and transitory water particles, in the aggregate visible through long distances, but probably individually beyond the power of the microscope to discern. They may be compared to the blue mist escaping from the safety-valve of a boiler under high pressure: the invisible steam turns for a moment blue, and then to the ordinary white of visible steam. The haze may possibly be equally momentary in duration, dissolving long before reaching the white stage, but fresh filaments are perpetually keeping up the process and giving the appearance of a persistence like that of smoke or dust. According to Espy, every cloud is either forming or dissolving (Buchan's "Handy Book of Meteorology," p. 175).

The action of a north-east wind setting in over England would be represented by a trough of water, say 2 feet square and 2 inches deep, containing warm water flowing in one direction, while cold water enters from the whole length of the opposite side. The cold water would force its way under the warm, and the two opposite currents would continue to flow; but through friction and diffusion there would be a great deal of mixture of portions of the upper with the lower stream.

A haze similar to that accompanying the east wind is frequently seen where two currents of the same wind meet at different temperatures, as at the junction of two valleys, or at projecting headlands (Buchan's "Handy Book of Meteorology," p. 171). It is also common with a humid wind, otherwise clear, when it passes over ranges of hill and valley of moderate elevation, owing probably to the mixture of parcels of air of different temperatures by alternate upward and downward thrusts. The thin white mist which appears in gales from the south-west on sunshiny days is probably due to the forcible and rapid mixture of air warmed by the ground with colder portions from a higher level, the deposition of minute particles of dew being aided by the abnormal amount of salt carried up from the sea in spray, and borne to great distances inland.

A very good instance of the powerful influence of the mixture of two currents of air, not greatly differing in temperature and other conditions, to produce haze occurred on August 26, 1889, in southern Surrey. The wind over a wide area, including the south of England, was variable and gentle from west to north-west. At the place of observation it had been about west-north-west during the afternoon, and the views were fairly clear. Cirro-cumulus, both at a moderate and at a great elevation, moved from north-west. At about 5.30 p.m. the landscape was suddenly invested with haze, which, during the following hour, was thick enough to obscure altogether hills about six miles off. Simultaneously the wind dropped a good deal and shifted to north-west and north for a short time, but soon backed, and the air again became clear about 7.30. It would thus seem sufficient that a reduction of temperature a little more than the ordinary about the time of sunset should occur, in order to precipitate visible moisture upon the dust-particles of the air. Both the sensation and the appearance of the sky resembled that during a disagreeable misty east wind, and, just before the change, a very dark bank of cloud appeared in the north, which, on passing over, was seen to be more mist than a well-defined cloud stratum. It seems not unlikely, judging from the experience of aëronauts, that in this case a current from north or north-east was driven like a wedge into the general north-west wind a few thousand feet or less above the ground.

If the account of the formation of haze in an easterly wind given in the foregoing pages be correct, there should be a clearing of the atmosphere when either the east wind extends itself to the upper regions or the westerly wind succeeds in driving back its opponent out of the lower space. In point of fact, the air does clear itself in either of these events. Moreover, a clearing away of haze is a good indication of a strengthening of the polar current or its expulsion by the equatorial; other signs, such as the motion of cirrus and the aspect of the clouds, plainly informing us which of the two changes will occur.

(2) The second favourable state for the production of haze was given as "fine settled weather, with variable currents, a dry air, and little dew." This state prevails often with anticyclones, and the movement of the air is to a great extent vertical, an interchange taking place between upper and lower strata. Consequently, there is a great mixture of portions of air at different temperatures, with a result like that already described. The heterogeneous character of the lower atmosphere in a horizontal direction declares itself by the poor transmission of sound. But a great deal remains to be explained in the production of haze in these conditions. The cause is probably the same as that which sometimes covers the whole of the British Isles with a damp fog, extending high into the atmosphere. This occurs when two winds of a different character meet in such a manner as to interdiffuse gradually over a wide area. But in the case of haze, how can it endure when the general dryness of the air is far above the point of saturation? Haze sometimes continues in summer right through the day, when the dry and wet bulbs show a difference of 12° to 15°. It would seem as if our methods of estimating the dew-point do not altogether hold for air in a certain condition and for certain particles in it. Is it not possible that condensation to a slight degree may occur upon some minute crystalline particles, such as the salt-dust which pervades our atmosphere, at temperatures above the dew-point? Such action would only be consistent with the effect of crystals in hastening the boiling and congelation of water. It is probable that, if means were available for testing the temperature of successive minute portions or strands of air passing over a thermometer, we should find a great variation from one moment to another. A difference of 12° between the dry and wet bulbs may represent a mean between much higher and much lower values; and on the driest days, when haze prevails, there may be extremely minute portions with a temperature at the dew-point—that is, containing more vapour than, at the particular temperature to which it is a certain moment exposed, can remain uncondensed. That volumes of air at different temperatures take a long time to become thoroughly incorporated, may be regarded as certain. Threads of smoke in a still room often remain for many minutes unbroken, and behave as if they were held together by some cohesive force, and, generally, strains of air or gas at widely differing temperatures, when mixed, tend to hold together rather than to diffuse. Thus, small surfaces, of which the vapour-particles are at different temperatures, are frequently in contact. When we consider that different currents of air frequently prevail within a a few thousand feet of the earth's surface, and that within five miles a temperature of $-2°$ may exist early in September,[1] it seems possible that, in so bad a conductor of heat as air, temperature at different points on the same level may vary greatly. On September 1 and 2, 1889, the condition of the air was instructive with regard to the formation of fog and haze. The night of August 31–September 1 was fine, and radiation rapid, so that in the morning there was a copious dew. From 6 to 8 a.m. there was thick fog, which, as the sun's power increased, lightened and lifted, but the sun did not finally break through till past 11. The wind was fresh from north-east. A thin blue haze remained after the fog had dissipated, and did not altogether disappear during the day. The air was not damp, even before the fog had lifted, though there was a very slight drizzle about 9 a.m. On September 2 the night had been very fine and clear, but in the morning

[1] See "Travels in the Air," Glaisher's ascent of September 5, 1862.

a thick wet fog, with fresh north-east wind, prevailed. This fog cleared, and the sun shone through, about 9 a.m. A mist, however, remained much later. Now, in these cases, the fog was due to the cooling of the earth by radiation (for it did not appear till after midnight) and to the cool north-east wind co-existing with higher currents from a different quarter.[1] The persistence of the haze much beyond the fog reveals the difference between a general saturation and what might be termed molecular saturation. The fog breaks, decreases rapidly, and has gone when the last few shreds of clouds lifted from the earth vanish in the blue, but the haze looks unchanging and uniform over the country. When we see volumes of vaporous air separated, without any apparent reason, into dense clouds and clear intervals, *e.g.* cumulus in a blue sky, it becomes easy to understand that very small microscopic clouds, in which condensation is only momentary, may permeate air otherwise far from saturation.

It would hardly be reasonable to exclude electricity as a possible agent in the otherwise not wholly accountable phenomena of mist and cloud. It may be that the dust-particles of two currents of air differing in electric quality or quantity may be attracted to each other, or that the mixture of currents of different temperature may in some way set up molecular aggregations.

Whatever the cause, we should bear in mind the small quantity of non-transparent matter required to produce the dimming effect of haze. If the eye can observe the colour produced in a drop of water by the fifty-millionth of a gramme of fuchsine, possibly a weight of water or dust not much greater would suffice for visibility in a column of air 1000 feet long. The atmosphere is at all times charged with dust-particles to a degree which it is difficult to realize. The purest air tested by Mr. Aitken previous to his measurements on the top of Ben Nevis, contained about 34,000 dust-particles to the cubic inch—this was on the Ayrshire coast. In every cubic foot there would be 35,232,000 particles, and, in a horizontal column of 1000 feet, 35,232,000,000 particles. It is manifest that a condensation upon a small proportion of these, or an agglomeration of a small proportion into larger groups, or a momentary adhesion by electric attraction, would suffice to produce optical effects.

The evidence concerning the appearance of haze by irregular transmission of light due to unequally heated currents of transparent air seems to be quite insufficient, and however great the heat near the surface of the ground, say in the desert, with consequent distortion of images, it does not, as a rule, bring about the haze so common in temperate climates.

Haze of an abnormal kind need barely be mentioned here—namely, that due to smoke, palpable dust, and the products of volcanoes. It may, however, be very widely spread and very dense. In 1783 Europe was for months covered by the dust ejected by an Icelandic volcano, and the Atlantic for 900 miles west of the north-west coast of Africa is every year subject to a haze composed of fine particles of sand from the Great Desert.

(3) Opposition of currents, such as takes place when several shallow barometric depressions pass over the country, results in mixture of differing air, partial condensation, sultriness, haziness, and frequently thunderstorms. Not at all improbably, the differing electric conditions of two winds, the rapid condensation of vapour, and the projection of highly vaporous air to a great height, accelerate the growth of water-particles, until they fall to the earth in large drops. The saying that thunderstorms advance against the wind is merely a way of asserting that two winds are adjacent, one above the other, and that the clouds move in the upper current. The haze preceding thunderstorms announces beforehand the contention which is going on, and the conglomeration of dust or water particles by electric attraction or rapid cooling.

(4) Damp weather with light winds and varying temperature, as thaw after frost, with snow on the ground. The cause of haze in this condition is obviously the contact of warm moist air with air cooled by contact with, and by radiation towards, the ground. In this case, again, it is mixture of portions of air of different temperatures which produces partial condensation and haze. It must be remembered that the air is always charged with an immense quantity of fine dust, such as particles of salt,[1] that these are capable of radiating, and that when they fall 1° or 2° below the temperature of the air, moisture may be deposited upon them sufficiently to become visible. In the case supposed, of an equatorial current supervening after frost and snow, the mist produced by mixture of parcels of air at different temperatures will be thin and blue if the filaments in which saturation and deposition occur are very small in proportion to the surrounding unsaturated air, and white if the proportion of saturated air is large. For the blue mist or haze indicates deposition in very minute clusters of water-molecules, and instant reversion to the invisible state by the contact of unsaturated air, while the white mist is the result of condensation in much larger quantities in air on the whole very near or at the point of saturation.

Consider next the conditions of weather in which the air is most transparent.

(1) A state of great humidity, such as that which occurs often before bad weather, the wind being between south and west. What does this clearness signify, according to the views of the causation of haze above detailed? Chiefly that the air up to a great height is fairly homogeneous—that is, of the same kind and quality as regards moisture, electricity, and temperature, with due allowance for the normal changes depending on altitude. The humidity is not owing to this homogeneity, but often accompanies it, simply because the south-west and westerly winds have passed over a large extent of ocean. In fact the air throughout has been subjected to the same influences, and nothing has occurred to disturb its uniformity, so that it can for some considerable time carry a large amount of aqueous vapour without precipitation. When precipitation does occur, it is usually by the thrusting upwards of the warmer strata into cold upper strata, and then condensation proceeds without check and rapidly from invisible particles to rain-drops. Thus, on reaching the first mountainous region, or in passing over land heated to a temperature much above that of the sea surface, the ascent of the most humid strata into the cold upper air is often followed by rain. The remarkable transparency before rain signifies a correspondence in direction as well as in qualities between the upper and lower strata. If the wind be between west and south, as it usually is in these cases, we are informed of a similar wind at a high level—that is, that the upper current, as well as the lower, is more than commonly humid, and its vapour tending to condense by passing towards higher latitudes. It only requires slight disturbances in a vertical direction to precipitate the abundant vapour, and hence the frequency of showers, especially where large columns of heated air rise from the land, at a distance from the south coast, and in hilly country. The south-westerly wind being a warm one, is more likely to ascend and to have its vapour condensed to rain than a colder current. The clear lower air indeed owes its clearness partly to its ascending movement.

(2) Strong winds and showery weather. Strong winds usually prevail when the air up to a great height partakes more or less of the same movement. There is

[1] "On Saturday evening, August 31, a balloon, as it ascended, crossed and recrossed Luton several times."—*Daily News*, September 2, 1889.

[1] Salt is shown to be present everywhere in the atmosphere by the spectrum of a flame.

also no opportunity for the filtering through of small portions of dissimilar air, and, if portions do descend into the lower levels, they are broken up, diffused, and dispersed. Still, in the colder half of the year, if the lower wind blows from between east and north, and does *not* extend to a great height, a strong mist may be produced by its being mixed with detached portions of the westerly upper current, which take a long time to be thoroughly incorporated and dissolved, and contain more vapour than they can hold invisible in contact with the cold surface-breeze. Thus the prevalence of much haze with a north-easterly gale indicates an equatorial upper current, and the polar wind is apt to be replaced by it before long. With regard to showery weather, it may almost be said to be the opposite of hazy weather, and for the following reasons:—First, as we have seen above, showers are produced by the upward projection of lower air, containing a good deal of vapour, into upper cold air of the same kind. Then, they are often the expression of a state of the atmosphere when the interchange between the upper and lower strata proceeds by large ascending columns and large down-rushes, instead of by small convection currents, and ascending and descending filaments over a very large area. The clearness of the air with a showery north-east wind is quite surprising, for it is sufficient to banish to a great extent even London smoke. Here, again, the north-east wind prevails to a great height, and the air is homogeneous and rather dry. When a shower or even a cumulus cloud passes over a large town, the smoke is seen to be drawn up in a moving column to the height of the cloud. Probably the chief cause of the clearness of a showery north-east wind is the prevalence, as in other cases, of the same wind in the upper regions, so that there is no admixture of strange threads in its composition, no strands of extra-humid particles to be rendered visible by incipient condensation.

(3) Winds between south-west and north. These are, on the whole, clear for a similar reason, for it has been shown that the upper currents in Great Britain usually move from between south-west and north-west. If, as occasionally happens, an east wind blows overhead, they are very far from transparent.

(4) Fine settled summer weather, with westerly or southerly winds, is clear not only for the reason above stated, but on account of the general moderate dryness of the atmosphere. In such weather, barometric pressure is frequently highest over Spain or France, and our upper currents are accordingly from north-west, becoming warmer as they advance southwards and increasing in capacity for moisture. There would be no condensation if portions of these currents were to descend into the lower air.

(5) Settled easterly or northerly winds, with either clear sky or high clouds moving from those directions. Haze does not form where the wind is steady, the air dry and homogeneous up to a great height, and equilibrium stable, for there is nothing to lead to condensation except at the particular level of saturation where clouds are manifested.

(6) Easterly or northerly winds with a high continuous cloud canopy moving in the same direction, small range of temperature, and steady conditions; or, with detached cumulus in the daytime, and clear nights. The same remarks apply here as to the last.

(7) North-west wind, reaching that point from west or south, is particularly clear. Great transparency in this case is not a sign of rain, but rather of fair weather. It is probably due to its agreement in general direction with upper currents, the increasing dryness as it reaches warmer latitudes, and to the uniformity and equilibrium attained by passing over the ocean.

F. A. R. RUSSELL.

THE PULSION MECHANICAL TELEPHONE.

(FROM A CORRESPONDENT.)

A NEW mechanical telephone of extraordinary power has recently been exciting considerable attention in London and some other cities and towns in this country. It is of American origin, like so many other modern improvements of exceptional character, being the invention of one Lemuel Mellett, I believe of Boston, U.S. There have been many previous mechanical telephones, as your readers are aware, some of which have obtained much publicity for a short time, and then have been heard of but little more; but having had opportunities of experimenting frequently with the new instrument, and observing its vocal power, so to speak, under very various circumstances, I cannot doubt that it has a great future before it.

It may be clearly stated at once that the pulsion instrument is absolutely independent of all electrical aids or appliances, and therefore needs neither battery power to bring it into play, nor insulation of any of its parts to keep them effective. It consists solely of two cheap and simple instruments connected by an ordinary non-insulated wire of copper, or, better still, of a double steel wire, the two parts being slightly intertwisted, say with about a single turn in a couple of feet. The wire (or wires) is simply looped to the instrument at either end, the connection being made in a few seconds. The instrument consists of a disk in combination with a series of small spiral springs inclosed in a case of some three or four inches in diameter. These springs, arranged in a manner that has been determined by experiment, and so as to produce harmonized vibrations, appear to possess the power of magnifying or accumulating upon the wire the vibrations which the voice sets up in the disk, and the wire seems to possess—undoubtedly does possess—the power of transmitting to great distances, and giving out upon a second pulsion instrument, the sounds of the voice.

The ability of this simple system of springs, disks, and wires to convey conversational and other sounds to considerable distances with great clearness and distinctness, reproducing the very tones of the voice and the qualities of musical sounds with but little reduction or modification, is most surprising, and to none more so than to the many men of science who have been recently experimenting with it.

The writer of this notice cannot, perhaps, do better than state his own experiences with this system. After examining and experimenting over several short lengths of wire, some of them exceeding a mile and a half, he last week went to the Finchley Road Station of the Midland Railway, from a point near to which a line had been conveyed to near the Welsh Harp Station, a distance of three miles by the line of railway, and of more by the track of the wire, which for the larger part was carried by the telegraph-posts, to which it was attached by very simple means. Conversation through this length of line, of over three miles, was exceedingly easy; indeed, so powerfully was the voice transmitted, that an ordinary hat sufficed for all the purposes of the second instrument, without going near to which conversation was carried on repeatedly by means of the hats of three gentlemen who were present, the tops of which were merely placed against the telephone wire.

I then went into the garden of the "Welsh Harp," where a short length of wire had been led between two points, the wire on its way from one point to the other being twice tightly twisted, at an interval of some yards, round small branches of trees, of about 1 inch in diameter, being wound round and round the branch three times in each case. Strange to say, this tight twisting of the wires round the branches in no way interfered with the transmission of the voice from end to end of the wire.

A third and last experiment was made with a wire laid obliquely across the Welsh Harp lake, and allowed to sink to, and rest upon, the lake bottom. The length of the line was roughly estimated at about one-third of a mile, and from end to end (excepting a few yards at each end where the wire was led from the water's edge to the telephone box) the wire was completely immersed, and without any other support than the bottom of the lake offered it. Yet, notwithstanding this immersion of the whole wire, conversation was carried on through it by means of the pulsion instruments without the least difficulty. In fact, the voice came through the immersed wire, and the longest wire (of over three miles) previously mentioned, with greater purity and mellowness than through shorter lengths.

I must leave to others to explain, and if necessary to discover, the scientific grounds of the success of this extraordinary little instrument. Looking, however, at its practical capabilities as exemplified above, it is not surprising that Post Office, police, railway, and other commercial people, are already overwhelming with applications those who are arranging to supply the new telephone, which from its extreme simplicity is manifestly a cheap one.

NOTES.

No fewer than 1810 patients bitten by dogs were treated at the Pasteur Institute in the year ending October 31. There were thirteen deaths.

THE *Daily Graphic*, the first number of which will appear on January 4, will be interesting from a scientific as well as from a popular point of view. Twenty years ago, when the *Graphic* was started, so bold an enterprise would have been impossible. At that time the pictures in illustrated journals were produced only by the old method of wood-engraving, which could not, of course, supply all the needs of a daily illustrated paper. By means of various scientific processes, drawings can now be so rapidly and effectively reproduced, that the issue even of a daily illustrated journal may be safely undertaken. The new paper is likely to afford a very striking instance of the influence of these processes on art and journalism.

THE Government of New South Wales has adopted an entirely new scheme of technical education. The present Board of Technical Education is to be abolished, and technical schools will be placed under the direct control of the Education Department. A sum of £50,000 is to be expended in the erection and equipment of a new Technical College and Museum in Sydney, while branch technical schools will be established throughout the country districts. It is estimated that £50,000 will be required annually to carry out the new arrangements.

MR. E. W. COLLIN has been deputed by the Government of Bengal to make inquiries as to the present condition of technical education in Bengal, and to find out what steps should be taken by the Government towards its advancement in that Presidency. The Civil Engineering College at Seebpore, an institution for the training of overseers and civil engineers, is supported by the Bengal Government, but it does not appear that there are any means at present in Bengal for the technical training of artisans. Mr. Collin has addressed a circular to various public bodies asking for information, and he will submit a report on the question about the end of the year.

MR. G. BERTIN is to deliver, at the British Museum, a series of four lectures on the religion of Babylonia. The first lecture will be given on November 26, and the others on the three following Tuesdays, at 2.30 p.m.

MR. G. B. SCOTT, of the Indian Survey Department, who has lately been employed on a survey of the Wards Estates in Bengal, has been placed in charge of the new Cadastral Survey of Upper Burmah.

THE next *conversazione* of the Royal Microscopical Society will be held on Wednesday, the 27th instant, at 8 o'clock.

MR. THOMAS CHILD, who has just returned from Pekin, has sent us very beautiful photographs of the two interesting old astronomical instruments at the Pekin Observatory. These instruments are the most ancient of the kind in the world, having been made by order of the Emperor Kublai Khan in the year 1279. They are exquisite pieces of bronze work, and are in splendid condition, although they have been exposed to the weather for more than 600 years. They were formerly up on the terrace, but were removed down to their present position to make way for the eight instruments that were made by the Jesuit Father Verbiest in 1670, during the reign of the Emperor K'ang Hsi, of the present dynasty.

THE metric system of weights and measures having been adopted in the Photographic Office of the Indian Survey, a series of tables for the conversion of these measures to British, and *vice versâ*, has been prepared by Colonels Thuillier and Waterhouse, Surveyor-General and Assistant-Surveyor-General of India. The scope of the tables, however, has been extended so as to meet, as far as possible, the ordinary requirements of general and scientific reference. The multiples and fractions of the British and metric units have each their equivalent expressed in the other, so that the number requiring to be converted may be multiplied directly by the decimal fraction representing the equivalent value of one unit of the required denomination. The relative equivalents are given for the conversion of measures of length, weight, and capacity, cubic and square measures, and also of British-Indian and metric weights. There are also a few miscellaneous tables that may be found generally useful.

IT is well known that whales can remain a long time under water, but exact data as to the time have been rather lacking. In his northern travels, Dr. Kückenthal, of Jena, recently observed that a harpooned white whale continued under water 45 minutes.

THE elephant skeleton set up in the front hall of the Madras Museum is 10 feet 6 inches high, and it has been stated that this is the skeleton of the largest elephant ever killed in India. Mr. Edgar Thurston, Superintendent of the Museum, in his latest Report, says that this is a mistake. Mr. Sanderson gave 10 feet 7½ inches as the largest elephant he had met, and there is a still larger one in the Indian Museum, Calcutta.

SOME fragments of a gigantic elephant's tusk (we learn from the *Rivista Sci. Ind.*) were lately obtained by Signor Terrenzi, the tusk having been found in the yellow Pliocene (marine) sands of Camartina, Narni. It must have been about 10 feet long. One piece (which seems to have been near the base) measured about 2 feet round at the thickest. The tusk had been broken up by the peasants, and distributed as an infallible remedy for tooth-ache and for belly pains in cattle! It probably belonged either to *E. meridionalis*, Nesti, or to *E. antiquus*, Falc. The finding of elephant remains in the Pliocene marine sands of Italy is not new, but it is rare.

A REMARKABLE paper on "The Ethnologic Affinity of the Ancient Etruscans," by Dr. Daniel G. Brinton, was read before the American Philosophical Society on October 18, and has now been issued separately. Dr. Brinton's attention was specially called to the subject during a sojourn of some months in Italy, early in the present year, when he had an opportunity of studying many museums of Etruscan antiquities. The object of the

paper is to prove that the Etruscans probably came from Northern Africa, and belonged to the same stock as the Kabyles, on the borders of whose country Dr. Brinton had spent some time before his visit to Italy. He thus sums up his conclusions:—(1) The uniform testimony of the ancient writers and of their own traditions asserts that the Etruscans came across the sea from the south, and established their first settlement on Italian soil near Tarquinii; this historic testimony is corroborated by the preponderance of archæologic evidence as yet brought forward. (2) Physically, the Etruscans were a people of lofty stature, of the blonde type, with dolichocephalic heads. In these traits they corresponded precisely with the blonde type of the ancient Libyans, represented by the modern Berbers and the Guanches, the only blonde people to the south. (3) In the position assigned to woman, and in the system of federal government, the Etruscans were totally different from the Greeks, Orientals, and Turanians; but were in entire accord with the Libyans. (4) The phonetics, grammatical plan, vocabulary, numerals, and proper names of the Etruscan tongue present many and close analogies with the Libyan dialects, ancient and modern. (5) Linguistic science, therefore, concurs with tradition, archæology, sociologic traits, and anthropologic evidence, in assigning a genetic relationship of the Etruscans to the Libyan family.

A LAKE-DWELLING has been discovered in the neighbourhood of Somma Lombardo, north-west of Milan, through the draining of the large turf moor of La Lagozza. The Berlin Correspondent of the *Standard*, who gives an account of the discovery, says that this "relic of civilization" was found under the peat-bog and the underlying layer of mud, the former being 1 metre in thickness, and the latter 35 centimetres. The building was rectangular, 80 metres long and 30 metres broad; and between the posts, which are still standing upright, lay beams and half-burnt planks, the latter having been made by splitting the trees, and without using a saw. Some trunks still retain the stumps of their lateral projecting branches, and they have probably served the purpose of ladders. The lower end of these posts, which have been driven into the clay soil, is more or less pointed, and it can be seen from the partly still well-preserved bark that the beams and planks are of white birch, pine, fir, and larch. Among other things were found polished stone hatchets, a few arrow-heads, flint knives, and unworked stones with traces of the action of fire.

MR. R. ETHERIDGE, JUN., contributes to the Report of the Australian Museum, just received, an interesting appendix on the limestone caves at Cave Flat, junction of the Murrumbidgee and Goodradigbee rivers, county of Harden. Having recorded the observations made by him in these remarkable caves, Mr. Etheridge offers some remarks on the Murrumbidgee limestone. This, he says, is of a dense blue-black colour. It is much jointed and fissured, highly brittle in places, with a hackly conchoidal fracture, and crammed with fossils, especially corals. As a display of these beautiful organisms in natural section, he has never seen its equal. Large faces of limestone may be seen, with the weathered corals, and particularly Stromatopora, standing out in relief and in section also. Many of these masses of coral, particularly those of Stromatopora and Favosites, are as much as 4 feet in diameter. The Murrumbidgee limestone has been classed as Devonian by the late Prof. de Koninck, but Mr. Etheridge has not yet sufficiently examined the fossils of this deposit either to gainsay or confirm this view. He thinks it not improbable, however, that Prof. de Koninck's view may be correct.

THE *Comptes rendus* of the Paris Academy of Sciences, of November 4, contains a note by M. A. Angot, on the mean hourly velocity of the wind at the summit of the Eiffel Tower, measured during 101 days, ending with October 1, by means of an anemometer placed at 994 feet above the ground, and compared with the results of a similar instrument at the Paris Meteorological Office, placed at 66 feet above the ground. The average velocity on the tower was 16 miles an hour, being over three times the amount registered at the Meteorological Office, where it was only 5 miles an hour. At the lower station the diurnal variation showed a single minimum about sunrise, and a single maximum about 1 h. p.m. On the tower the minimum occurred about 10h. a.m., and the maximum about 11h. p.m., while the characteristic maximum of lower regions about the middle of the day was hardly perceptible on the tower. It is remarkable that this inversion, which is usual upon high mountains, should occur at so small a height as that of the Eiffel Tower. The ratio of increased velocity was constant at about 5 : 1 between midnight and 5h. a.m.; it then decreased rapidly and became 2 : 1 at about 10h. a.m., and maintained this value until 2h. or 3h. p.m., when it again rose regularly until midnight. These results are of considerable importance to the study of aërial navigation.

THE new number of the *Mineralogical Magazine* opens with an important paper, by Mr. L. Fletcher, F.R.S., on the meteorites which have been found in the desert of Atacama and its neighbourhood. This paper is accompanied by a map of the district. Prof. McKenny Hughes, F.R.S., has a paper on the manner of occurrence of Beekite and its bearing upon the origin of siliceous beds of Palæolithic age. There are also three short papers by Dr. M. F. Heddle, and one by Mr. R. H. Solly.

SOME experiments on the photography of the red end of the spectrum, by Colonel J. Waterhouse, appear in the Proceedings of the Asiatic Society of Bengal for April 1889. In order to render the ordinary commercial gelatine dry plates sensitive to the red rays they are bathed for one or two minutes in a solution of 1 part of alizarin blue ($C_{17}.H_9.NO_4$) to 10,000 parts of distilled water with 1 per cent. of strong ammonia added. Plates treated with this dye show very intense action through the violet and blue regions as far as *b*; from E to C there appears to be a minimum of action; the sensitiveness, however, increases between C and A, and is strongest between C and B and a to A. Below A the sensitiveness quickly diminishes. Colonel Waterhouse finds that plates saturated with a special preparation of cyanin and sulphate of quinine have their maximum sensitiveness between D and B, but between B and A the action is much weaker than that obtained by using alizarin blue, hence the latter dye is valuable as a ready and simple means of photographing the spectrum between C and A with ordinary dry plates. For orthochromatic photography, rhodomine was found to be almost as efficient as erythrosin, and to be especially useful for photographing the region immediately about D. The photographs were taken by means of Rowland's plane and concave diffraction gratings.

A NEW mode of preparing manganese, by which the metal can be obtained in a few minutes in tolerably large quantities and almost perfectly pure, is described by Dr. Glatzel, of Breslau, in the current number of the *Berichte*. A quantity of manganous chloride is first dehydrated by ignition in a porcelain dish, and the pulverized anhydrous salt afterwards intimately mixed with twice its weight of well-dried potassium chloride. The mixture is then closely packed into a Hessian crucible and fused in a furnace at the lowest possible temperature, not sufficient to volatilize either of the chlorides. A quantity of metallic magnesium is then introduced in small portions at a time, the total quantity necessary being about a sixth of the weight of the manganous chloride employed. Provided the crucible has not been heated too much above the melting-point of the mixture of chlorides, the action is regular, the magnesium dissolving with

merely a slight hissing. If, however, the mixture has been heated till vapours have begun to make their appearance, the reaction is extremely violent. It is therefore best to allow the contents of the crucible, after fusion, to cool down to a low red heat, when the introduction of the magnesium is perfectly safe. When all action has ceased, the contents of the crucible are again heated strongly, and afterwards allowed to cool until the furnace has become quite cold. On breaking the crucible, all the potassium chloride and the excess of manganous chloride is found to have been volatilized, leaving a regulus of metallic manganese, fused together into a solid block, about three parts by weight being obtained for every two parts of magnesium added. The metal, as thus obtained, is readily broken up by hammering into fragments of a whitish-gray colour possessing a bright metallic lustre. The lustre may be preserved for months in stoppered glass vessels, but, when exposed to air, the fresh surface becomes rapidly brown. The metal is so hard that the best files are incapable of making any impression upon it. It is so feebly magnetic that a powerful horse-shoe magnet capable of readily lifting a kilogram of iron has no appreciable effect upon the smallest fragment. It was noticed that the introduction of a small quantity of silica rendered the manganese still more brittle, and caused it to present a conchoidal fracture, that of pure manganese being uneven. The specific gravity of the metal, former determinations of which have been very varied, was found to be 7·3921 at 22° C. This number, which was obtained with a very pure preparation, is about the mean of the previous determinations. Dilute mineral acids readily dissolve the pulverized metal, leaving a mere trace of insoluble impurity. It is also satisfactory that practically no magnesium is retained alloyed with the manganese, and the introduction of carbon is altogether avoided by the use of this convenient method.

The additions to the Zoological Society's Gardens during the past week include a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mr. O. Burrell; a Common Squirrel (*Sciurus vulgaris*), British, presented by Miss B. Tatham; a Common Stoat (*Mustela erminea*) from Northamptonshire, presented by Mr. Cuthbert Johnson; a Wattled Crane (*Grus carunculata*) from West Africa, presented by Mr. Robert Sinclair, Jun.; a Redshank (*Totanus calidris*) from Devonshire, presented by Mr. R. M. J. Teil; a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, presented by Mr. W. H. Felstead; a Grey-headed Porphyrio (*Porphyrio poliocephalus*) from India, presented by Dr. Gerard Smith; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. G. W. Alder; a Dwarf Chameleon (*Chamæleon pumilus*) from South Africa, presented by Mrs. Leith; a Green Lizard (*Lacerta viridis*), European, presented by Mr. C. H. Whitlow; a Common Jay (*Garrulus glandarius*), European, purchased; five Carpet Snakes (*Morelia variegata*) from Australia, received in exchange.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m., November 21 = 2h. 3m. 21s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G. C. 527	—	—	2 15 28	+41 35
(2) 15 Arietis	6	Yellowish-red.	2 4 31	+18 59
(3) α Arietis...	2	Yellow.	2 1 0	+22 57
(4) β Trianguli	3	Bluish-white.	2 3 0	+34 28
(5) DM + 56° 724 ...	9	Reddish-yellow.	2 42 32	+56 31
(6) R Tauri	Var.	Very red.	4 22 16	+ 9 55

Remarks.

(1) Sir John Herschel's description of this nebula is as follows:—! Bright, very large, very much extended. The spectrum has not yet been recorded.

(2) This is a star of Group II., in which Dunér records bands 2–8, but states that they are neither wide nor dark. The star falls in species 13 of the subdivision of this group, and is well advanced towards Group III. Metallic lines, and possibly hydrogen lines (dark) may therefore be expected. In the earlier stages of the group, no hydrogen lines appear, the radiation from the interspaces between the meteorites being balanced by the absorption of the gas surrounding the incandescent stones; but in the more advanced members, as in α Orionis, the absorption will probably be found to slightly predominate. The presence or absence of the F line, and of metallic lines, and their relative intensities, should therefore be noted.

(3) This is a star of either Group III. or Group V., and the usual criteria (see p. 20) should be observed in order to determine which. At the same time, the relative intensities of the hydrogen lines and the metallic lines (say *b* and D) should be recorded, so that the star may be placed in a line of temperature with others.

(4) According to Gothard this is a star of Group IV. The usual observations are required.

(5) Dunér classes this with Group VI. stars, but states that the type of spectrum is a little doubtful. Further observations are therefore required. As the most advanced stars of the group are very red, the colour of this star indicates that it probably belongs to an early stage of the group, in which the carbon bands would be narrow, and therefore somewhat difficult to observe with certainty; in that case traces of *b* and D might be expected. The colour should also be checked.

(6) Gore gives the period of this variable as 325·6 days, and the range as 7·4–9·0 at maximum to < 13 at minimum. The maximum will occur on November 30. The spectrum is of the Group II. type, and belongs to species 9. Dunér states that the dark bands, especially 7 and 8, are very wide. In several variables of this class (R Leonis, R Andromedæ, &c.), Espin has observed bright hydrogen lines near maximum, and the question is, Is this common to all the variable stars of this type? As stated with reference to 15 Arietis, under normal conditions the hydrogen lines in the earlier species of the group are absent, because the interspacial radiation balances the absorption; but if through some cause the temperature increases at maximum, more hydrogen would be driven into the interspaces and radiation would predominate. It may be mentioned that, according to the meteoritic theory, the increase of temperature and luminosity is brought about by the periastrion passage of a secondary swarm through the outliers of the central one. It is not unlikely that slight variations of colour will take place from maximum to minimum, and it is important therefore that the colour should be noted when the spectroscopic observations are made.

A. Fowler.

The Minimum Sun-spot Period.—M. Bruguière, in *L'Astronomie*, November 1889, gives a series of observations made with a view to determine the exact date of the minimum sun-spot period. The following tables show the condition of the sun's surface with respect to spots from the beginning of January to the end of July of this year:—

Date, 1889.		No. of days without spots.	
Jan.	3–15		13
,,	18–31		14
Feb.	8–21		14
Mar.	2–6		5
,,	17–31		15
April	11–30	20	25
May	1–5	5	
,,	10–26		17
,,	28–31	3	18
June	1–15	15	
,,	29–30	2	13
July	1–11	11	
,,	25–27		3

Date, 1889.		No. of days with spots.
Jan.	16–17	2
Feb.	1–7	7
,,	22–29	8
Mar.	1 and 8–16 ...	10
April	1–10	10
May	6–9	4
,,	27	1
June	16–28	13*
July	12–24	13*
,,	28–31	4

* The same spot.

If the small spots that were seen from May 6–9, and also on May 27, be neglected, it will be seen that there would be a period without spots extending from April 11 to June 15—that

is, sixty-six days; but if these small spots be considered we find an interval of twenty-five days without spots—namely, from April 11 to May 5. The minimum period, therefore, appears to have passed about the end of April, this being the time when the greatest number of days passed without spots being observed on the sun. The new period opened with the appearance of a large spot on June 16.

RETURN OF BRORSEN'S COMET.—The following elements and ephemeris for this comet are given by Dr. E. Lamp in *Astronomische Nachrichten*, No. 2933:—

T = 1890 February 24·1358 Berlin midnight.

$$\left.\begin{array}{l} \omega = 14^\circ\ 55'\ 35''\cdot 89 \\ \Omega = 101\ 27\ 33\cdot 74 \\ \iota = 29\ 23\ 48\cdot 25 \end{array}\right\} \text{Mean Eq. } 1890\cdot 0$$

$$\phi = 54\ 7\ 46\cdot 19$$

$$\mu = 650''\cdot 3693$$

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	1889.	R.A. h. m. s.	Decl. ° ′
Nov. 21	22 9 17	−45 4·6	Dec. 11	22 26 40	−39 42·8
22	9 47	−44 50·3	12	27 55	−39 24 6
23	10 19	−44 35·8	13	29 11	−39 6·1
24	10 54	−44 21·2	14	30 30	−38 47·4
25	11 31	−44 6·4	15	31 50	−38 28·5
26	12 11	−43 51·4	16	33 12	−38 9·3
27	12 53	−43 36·2	17	34 36	−37 49 8
28	13 38	−43 20·8	18	36 2	−37 30·1
29	14 25	−43 5·2	19	37 29	−37 10·1
30	15 14	−42 49·4	20	38 58	−36 49·8
Dec. 1	16 6	−42 33·4	21	40 29	−36 29·2
2	17 0	−42 17·3	22	42 1	−36 8·4
3	17 56	−42 0·9	23	43 35	−35 47·2
4	18 54	−41 44·4	24	45 10	−35 25·8
5	19 54	−41 27·7	25	46 47	−35 4·1
6	20 57	−41 10·8	26	48 26	−34 42·0
7	22 1	−40 53·6	27	50 6	−34 19 6
8	23 8	−40 36·3	28	51 48	−33 56·8
9	24 16	−40 8·7	29	53 32	−33 33·6
10	25 27	−40 0·9	30	55 17	−33 10 0

THE COMPANION OF η PEGASI.—A companion to η Pegasi was discovered by Sir William Herschel in 1780, and subsequently observed by South in 1824. Its magnitude has been rated from twelve to fifteen. Mr. S. W. Burnham, however, notes (*Astronomische Nachrichten*, No. 2933) that, using the 36-inch refractor at the Lick Observatory, the Herschel companion appears as a close double. South's mean of two measures is given in his catalogue as:—

$$1824\cdot 84 \qquad 338^\circ\cdot 9 \qquad 89''\cdot 82 \qquad 2n\ \text{S.}$$

The following is the mean of four measures made at Mount Hamilton:—

η Pegasi.

B and C.					A and BC.		
1889·53	83°·3	0″·29	10·1	10·1	1889·53	339°·0	90″·38

The close pair is difficult, and can hardly fail to be a physical system, and Mr. Burnham thinks that, although it is not a test for the large telescope, it will not be seen with any small instrument.

GENERAL BIBLIOGRAPHY OF ASTRONOMY.—The second part of Vol. I. of this comprehensive bibliography has been published. It represents Houzeau's last work, and hence it is well that his biographical note, by A. Lancaster, should be included. The first part of Vol. I., published in 1887, contained the references to historical works and those relating to astrology; the part just published contains the references to biographies of astronomers and their epistolary communications, general astronomical works, astronomical societies and their proceedings, and everything relating to spherical astronomy. Works on theoretical astronomy are also well represented. The third and last part of Vol. I. is now in press, and contains references to all the published matter on the mechanism of the heavens, physical, practical, and descriptive astronomy, and the systems of cosmogony. The utility of this bibliography, when completed, needs no comment.

J. C. HOUZEAU'S "VADE MECUM."—With reference to our biographical note on J. C. Houzeau (p. 20), M. A. Lancaster writes to remind us that Houzeau's "Vade Mecum" was issued after the appearance of the second volume of the "Bibliographie Générale de l'Astronomie," the publication of which began in 1879. Moreover, the "Vade Mecum" was only a second edition of the "Répertoire des Constantes de l'Astronomie," inserted in 1877 in the first volume of the new series of the "Annales Astronomiques" of the Brussels Royal Observatory. The numerous materials brought together for the "Bibliographie Générale" suggested to Houzeau the idea of issuing a new edition of the "Répertoire" considerably corrected and enlarged.

A NEW COMET.—A new comet was discovered on November 17 by Mr. Lewis Swift, of the Warner Observatory, Rochester, New York. Place at November 17, 6h. 35m. 2s. G.M.T.; R.A. = 22h. 42m. 24s.; N.P.D. = 78° 9′. Daily motion in R.A., + 2m.; in N.P.D., − 15′. The comet was only faint.

MIRAGE IN THE SOUTH AMERICAN PAMPAS.

I WAS staying in the Pampas of the Argentine Republic, near Melincue, a small town of the Province of Santa Fe, from September 1888 to March 1889. During my stay I had the opportunity of observing certain mirage phenomena. It is possible that my notes may contain something of interest. They were, designedly, taken without reference to any previous knowledge of the theory of mirage that I might possess.

To illustrate my observations I had drawn *eight* diagrams; but, for the purpose of insertion in NATURE, I have been obliged to reduce these to *two*. Hence I fear that my descriptions may not be as clear as I should wish.

The most general conclusion at which I arrived was that there were *two* classes of mirage of very different character. The one I shall call "the summer mirage," the other "the winter mirage." I would observe that, without a telescope of some sort, one would be unable to make observations of much value; and that, as I had but a binocular telescope, in many details I failed to make out as much as I could had I possessed a larger telescope steadily mounted.

I. *The Summer Mirage.*

(1) This mirage is seen in full day. I was told that in normal years it is most remarkable in the extreme heat of summer. The summer of December, January, and February 1888 and 1889 was abnormally wet, however. And I myself saw the mirage most frequently in spring (September, October, and the earlier part of November), the grass being then short and very dry. Later on the grass became very long, and unusually green and damp, owing to the heavy rains. And then I saw the mirage but rarely in the grass plains, though on the several occasions on which I passed, in the blaze of a summer day, the dry sandy bed of an old laguna, the mirage was there to be seen very clearly.

On one or two occasions in spring I saw the mirage when there was a fairly cold wind and no perceptible sunshine, but still in full day.

(2) This kind of mirage usually appeared as a strip of "water" running more or less parallel to the horizon, at one end narrowing to a point, and at the other end opening out into the sky. It appeared much as an arm of the sea, or an estuary, seen near the horizon, and running parallel to it. The "water" was of the same colour as the sky above it near the horizon.

(3) Viewed through glasses, the whole of the land seen above and beyond the "water," the "water" itself, and to a less extent the land seen just this side of it, appeared wavy and ill-defined, flocculent, and (when there was any breeze) possessed of a drifting movement down the wind. At the thin end of the "water," and just beyond it in the line of the layer, one could see broken fragments of "water" drifting over the land; and, in like manner, the peninsula of land appeared to end in a line of drifting fragments.

(4) It appeared to me that the land seen beyond the watery layer was either within the limits of the natural horizon, or not much beyond them. One did not, as one did in the "winter mirage," see houses, &c., that were normally out of sight.

(5) Cattle, &c., seen in the watery layer were ill-defined. But on the whole it seemed that their legs were hidden, and bodies were reflected inverted, much as if they had been standing in shallow water.

(6) When I mounted higher, a mirage, if seen at all, was further off than when I stood lower.

If, when looking at the watery layer of a mirage, I mounted higher, the "water" narrowed, and the strip of land beyond it widened, until at a certain height of my head the "water" had narrowed into a wavy line of fragments. Further mounting caused the "water" to disappear. If, on the contrary, I stooped, the "water" appeared to widen, the strip of land above it to narrow, until at last the mirage joined the sky.

On one occasion, when the mirage was about a mile and a half distant, and on another occasion when about 250 yards distant, I caused the "water" to appear and disappear by a vertical movement of my head not exceeding 1 foot.

(7) Objects situated in the watery layer but rising out of it, or on the strip of land beyond it, were reflected in the "water" much as in true water; but all was ill-defined, and the inverted reflections often broken and lengthened.

(8) It appeared to me that objects on the strip of land beyond the watery layer were also to be seen faintly reflected in the land that lay between them and the 'water." And when, as in (6), I had raised my head until the "water" had just dwindled away, objects near the horizon were reflected inverted in the region from which "water" had vanished.

(9) By the aid of my glasses I came to the conclusion that objects were not really, as they appeared to the naked eye, "drawn up" by the mirage. But it seemed rather that, an

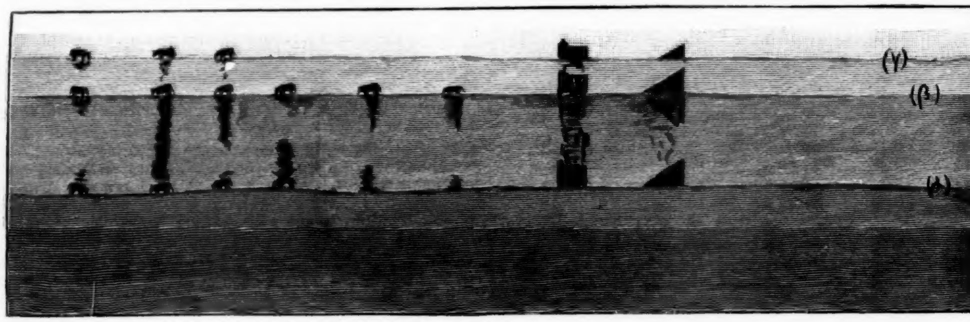

FIG. 1.

object being seen above its (often elongated) reflected image, and both being ill-defined, to the naked eye the whole appeared like the object "drawn up." In this way clumps of grass appeared as trees.

(10) In (1) I have mentioned the usual form of the mirage. But with various slopes, &c., of the ground, the form of the mirage varied. Sometimes the "water" opened out into the sky both ways; and several times I saw an isolated patch of "water" over an isolated patch of bare hot earth.

Conclusions as to Summer Mirage.—It seemed, then—

(1) That this mirage was due to a layer of relatively warm air close to the earth.

(2) That this mirage-giving layer was not more than about 2 feet in depth, and that it may have been less.

(3) That there were not, to any noticeable extent, vertical elongations of objects nor extensions of normal horizon.

(4) That in this mirage there were no images, erect or inverted, seen above the real object.

In fact, it seemed that the sky and terrestrial objects were simply reflected in a sheet of warmer air lying close to the ground. (Of course the paths of the rays would be curved.)

II. *The Winter Mirage.*

[I was told that this mirage is seen in winter, and best on fine mornings after hard frost. What I saw were, it seemed, but poor specimens.]

(1) I saw this mirage several times, always about sunrise and after a frost. Before sunrise, as soon as there was any light, I

FIG. 2.

looked out into the plains with my binoculars. It appeared as if the horizon were higher than usual, and that one could see tracts of land, with houses and other objects, that were usually concealed below the horizon.

Further, it seemed that this extension of horizon was not really continuous, as it at first appeared, but that it was divided into layers. As far as I could judge, the line (α) was beyond the normal limits of the horizon, the tract from (α) to the limit (β) was more or less a repetition of the tract below (α), and from (β) to (γ) was again more or less a repetition of the same tract. As to what one could see above the line (γ), I could make no trustworthy observations.

Before sunrise, this extension of the horizon was seen all round; and, though the layers referred to could be distinguished fairly well, there were as yet no "watery layers" to be seen.

The land seen just above the lines (α) and (β) was paler than that seen just below these lines.

(2) Thanks to a most convenient distribution of cattle of various colours, and of other objects, I was able, with the aid of my glasses, to make out a good deal.

But the images changed as the cows moved, the appearances varied as time went on, and were so different in different parts of the horizon, that I could only arrive at some general conclusions.

There would be, for example, just below, or on the edge of, the line (α), a cow. This I will call the "*first cow*," or the

"original cow." Just below or on the line (β), vertically above the *first cow*, and, like it, erect, would be a *second cow*, a repetition of the first. And often, above this again, below or on the line (γ), would be a third cow, also erect.

Sometimes there were confused images hanging from the *second cow* and joining other confused images piled on the *first cow*; sometimes the first cow was clear of images, while they hung down from the second cow; sometimes the second cow was clear, and there were images piled on the first. Often the *third cow* was missing (see Fig. 1). As the original cow moved, these images changed their disposition or vanished, and the third cow appeared or vanished. But in all these changes it seemed to me that the *first cow*, *second cow*, and (when visible) the *third cow*, were the permanent images. These, it appeared, were always erect.

(3) After the sun had risen, all continued *in statu quo* for a short time. But soon, at various parts of the horizon, the land just above the edges (α), (β), and (γ) paled away, and finally melted into the appearance of "sky" or "water." There were left, in the later stages of the mirage, first, the plain itself, with an extension, the limits of which were not sharp, beyond the normal horizon; secondly, above this a strip of land, apparently suspended in the air; thirdly, in some parts of the horizon another strip of land suspended in the air above this again. The interval between (α) and (β) was in all stages greater than that between (β) and (γ). One of the appearances in the later stages is indicated in Fig. 2.

Other changes crept in, too. Very often the original objects were wholly or partly sunk out of sight; the images were less defined; and the confused images hanging from the *second cow*, *e.g.*, or piled on the *first cow*, were now seen in the watery layers, sometimes bridging it over.

(4) As time went on, the watery layers widened. The images, too, became still vaguer, and the original objects were usually out of sight or only just indicated above the line (α). Moreover, the aërial images, with their confused trails of images hanging from them, began to assume more the appearance of "inverted images suspended over objects hidden below the horizon."

(5) In these later stages, no doubt, anyone would have guessed that the aërial images were indeed very vaguely defined *inverted* images. But to me, as I followed the phenomenon from the beginning, it seemed that they were not so. It seemed to me that each aërial image was really topped by an *erect* image, which, with the trails hanging from it, seemed like an inverted image. At least I can say that, so long as the images were well defined at all, I never made out a clear case of the *main*, or permanent, aërial images being inverted. Thus, as the *first cow* moved, it was the erect *second* (and sometimes *third*) *cows* that remained clear.

(6) In these later stages it was only trees and houses that could be seen in the mirage, and these were ill-defined.

(7) The mirage lasted until about an hour and a quarter after sunrise. The last traces of aërial images of land appeared just under the sun, and in that part of the horizon that lay just opposite to it. Whether the abnormal extension of the horizon entirely ceased at the same time, I cannot say; but there did not remain any noticeable extension.

(8) As with the summer mirage, I found I could alter appearances by altering my level above the earth. But the change in level had to be more considerable. I have no good notes on this matter; but I believe that usually I could recover a past stage of the mirage by a sufficient descent down a ladder from my post of observation.

General Conclusions as to Winter Mirage:—

(1) It is due to the earth, and the air near it, being considerably chilled below the temperature of the rest of the atmosphere.

(2) The phenomena of extended horizon and multiple images are to be observed.

(3) The "drawn up" appearance of objects is really due to a number of images piled upon one another, only to be separated by the use of a telescope.

(4) No case of a terrestrial object having above it a single inverted image, or images of which the uppermost was inverted, came under my notice. W. LARDEN.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xii. No. 1, and index to vols. i.–x. (Baltimore, 1889).—This volume opens with an instalment of sixty pages of a memoir by A. R. Forsyth, F.R.S., on "Systems of Ternariants that are Algebraically Complete." In this the writer has found it convenient to use "'ternariant' as a generic term for concomitants of ternary quantics, instead of giving it the signification which Prof. Sylvester proposed (*Amer. J. of Math.*, vol. v. p. 81) to give to it, viz. the leading coefficients of those concomitants." The memoir is divided into three parts, and deals with the theory of the algebraically independent concomitants of ternary quantics, taking as the starting-point the six linear partial differential equations of the first order satisfied by them. References are supplied to numerous memoirs on the subject.—Captain (now Major) P. A. Macmahon continues (pp. 61–102) his investigations (vol. xi. No. 1) in a "Second Memoir on a New Theory of Symmetric Functions." Herein he is engaged with functions which are not necessarily integral, but require partitions, with positive, zero, and negative parts for their symbolical expression. The author thus summarizes his results: (1) a simple proof of a generalized Vandermonde-Waring power law which presents itself in the guise of an invariantive property of a transcendental transformation; (2) the law of "groups of separations"; (3) the fundamental law of algebraic reciprocity; (4) the fundamental law of algebraic expressibility which asserts that certain indicated symmetric functions can be exhibited as linear functions of the separations of any given partition; (5) the existence is established of a pair of symmetrical tables in association with every partition into positive, zero, and negative parts, of every number, positive, zero, or negative.—The closing portion of the number (pp. 103–114) is taken up with an article entitled "De l'Homographie en Mécanique," by P. Appell.—A likeness of M. Poincaré faces p. 1.—The index is of a twofold description—of authors and of subjects. From the forewords we learn that papers have been published from eighty-nine contributors; these comprise "most of the leading mathematicians of the world."

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, November 11.—M. Hermite in the chair.—Presentation of Report of Proceedings of the permanent International Committee for preparing a photographic chart of the heavens, by M. E. Mouchez. Fifteen Observatories will be ready by the middle of next year; and five others before the end. The zones are indicated.—Note of M. Daubrée with descriptive catalogue of the meteorites of Mexico prepared by M. Antonio del Castillo. Meteorites are abundant in Mexico. A remarkably wide area of dispersion is indicated by three portions of one mass, found at the angles of a triangle, whose two longer sides were 90 km. and 60 km. In one of these places two plates were found 250 m. apart; and they seem to have formed one huge plate over 24,000 kgm. weight, which broke near the ground.—On the incineration of vegetable matters, by M. G. Lechartier. Trying various methods, he finds, that in the carbonization and incineration of a plant, there is considerable loss of sulphur, volatilized in various combinations; and special precautions are necessary in determining this constituent. Under the same conditions, and care being taken to prevent loss of solid matter carried away mechanically with the issuing gas, there is no sensible loss of phosphorus.—M. Picard was elected member in Geometry, in place of the late M. Halphen.—On a rotating magnetic field formed with two Ruhmkorff coils, by M. W. De Fonvielle. A current from accumulators is sent through the primary of one coil, the secondary of which is connected with that of the other coil, which is in a line with the first, and the primary of which may be open or closed.—On certain ellipsoidal areas, by M. G. Humbert.—On a new calculating machine, by M. L. Bollée. While in previous machines, multiplications, *e.g.*, are done by successive additions, this one has a multiplying apparatus which determines immediately, in one function, the product of a number by each figure of the multiplier.—On the solubility of the chlorides of potassium and of sodium in the same solution, by M. Étard. The results of experiment are shown in graphic form; the curves of solubility of each salt separately being compared with those of the mixed salts, &c. The sum of the dissolved salts is represented by a continuous straight line. The curves for the mixed salts cross at temperature 97°; that for NaCl falling while the other rises.—On an application of thermo-chemistry, by M. A.

Colson. The formation of nicotine monohydrochloride liberates about twice as much heat as that of the dihydrochloride under like conditions; hence a probable difference in constitution of the two nitrogen groups of nicotine. The action of nicotine on coloured reagents shows at once a difference in the two basicities.—On the myelocytes of fishes, by M. J. Chatin. In fishes, as in other zoological groups, the nervous elements termed myelocytes, are not to be referred to a special histic type, but to the nerve cell; which is simply modified, chiefly by enlargement of the nucleus, and corresponding reduction of the somatic part.—On the continuity of the pigmented epithelium of the retina with the external segments of the cones and rods, and the morphological value of this arrangement in vertebrates, by MM. R. Dubois and J. Renaut. This new fact makes it probable (according to the authors) that in the retina of vertebrates a similar process occurs to that in the light-sensitive apparatus of Mollusks like *Pholas*; by mechanism of impression and transformation of luminous movement into contractile, then sensorial.—On strabismus, by M. H. Parinaud. The immediate cause of the deviation (in squinting) is a disorder of innervation, excess in convergence, defect in divergence, caused generally by the accommodative effort in one case (hypermetropia), and the little use made of accommodation in the other (myopia). The deviation, when sufficiently fixed and prolonged, induces anatomical changes both in the brain-connections and the tissues of the eye (in the latter case, not only shortening of muscles, but retraction of all relaxed fibrous parts, especially Tenon's capsule). This has important bearings on treatment.—On the morphology and the biology of the fungus *Oidium albicans* (Robin), by MM. G. Linossier and G. Roux. Besides the *yeast* form, and the *globulofilamentous*, he finds a third, similar to *chlamydospores*, and probably needing some new natural habitat for full development. This fact, with the absence of *ascospores*, &c., suggests removal of the organism from the genus *Saccharomyces*. Again, it is found, that in culture of the fungus, the complication of form increases with the molecular weight of the aliment; there is a growing tendency to form long thin filaments. This tendency is also favoured by high temperature, excess of oxygen, a trace of nitrates, and antiseptics.—Comparative activity of various digitalines, by M. Bardet. He compares crystallized and amorphous digitaline, prepared according to the French codex, German *digitoxine*, French *digitaleine*, and German *digitaline* (the power of the two last is much less than those of the others).

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 21.

ROYAL SOCIETY, at 4.30.—(1) Further Discussion of the Sun-spot Observations at South Kensington; (2) on the Cause of Variability in Condensing Swarms of Meteorites: J. Norman Lockyer, F.R.S.—On the Local Paralysis of Peripheral Ganglia, and on the Connection of Different Classes of Nerve Fibres with them: J. N. Langley, F.R.S., and W. Lee Dickinson.—On the Tubercles on the Roots of Leguminous Plants, with Special Reference to the Pea and the Bean (Preliminary Paper): Prof. H. M. Ward, F.R.S.

LINNEAN SOCIETY, at 8.—External Anatomical Characters indicating Sex in Chrysalids, and Development of the Azygos Oviduct and its Accessory Organs in Vanessa Io: Prof. W. Hatchett Jackson.—Anatomy of Lepidoptera: E. B. Poulton.—Lepidoptera of Ichang, North China: John H. Leech.

CHEMICAL SOCIETY, at 8.—The Law of the Freezing-points of Solutions: S. U. Pickering.

MONDAY, NOVEMBER 25.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—The Bahrein Islands, Persian Gulf: J. Theodore Bent.

SOCIETY OF ARTS, at 8.—Modern Developments of Bread-making: William Jago.

TUESDAY, NOVEMBER 26.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—The Ethnology of the Western Tribe of Torres Straits: Prof. A. C. Haddon.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Water-Tube Steam-Boilers for Marine Engines: John I. Thornycroft. (Discussion.)

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—A New Genus of Polychæt Worm: Florence Buchanan.

WEDNESDAY, NOVEMBER 27.

SOCIETY OF ARTS, at 8.—Scientific and Technical Instruction in Elementary Schools: Dr. J. Hall Gladstone, F.R.S.

THURSDAY, NOVEMBER 27.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Electrical Engineering in America: G. L. Addenbrooke.

FRIDAY, NOVEMBER 29.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Principles of Iron Foundry Practice: G. H. Sheffield.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Pubblicazioni del Real Osservatorio di Palermo, vol. iv. (Palermo).—Obeah; Witchcraft in the West Indies: H. J. Bell (Low).—Through Atolls and Islands in the Great South Sea: F. J. Moss (Low).—The Lesser Antilles: O. T. Bulkeley (Low).—Humanitism: W. A. Macdonald (Trübner).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, vol. ii., 4th series (Manchester).—Report on the Mining Industry of New Zealand, 1889 (Wellington).—Reports on Mining Machinery and Treatment of Ores in Australian Colonies and America (Wellington).—Die Labyrinthodonten der schwäbischen Trias: E. Fraas (Stuttgart, E. Schweizerbart'sche).—The Butterfly; its History, &c.: J. Stuttard (Unwin).—A Glossary of Biological, Anatomical, and Physiological Terms: T. Dunman and V. H. W. Wingrave (Griffith, Farran).—An Introduction to the Study of Shakespeare: Dr. H. Corson (Boston, Heath).—On the Animal Alkaloids: Sir W. Aitken, 2nd edition (Lewis).—Matebele Land and the Victoria Falls, 2nd edition; F. Oates, edited by C. G. Oates (K. Paul).—Euclid's Elements of Geometry, books i. and ii.: H. M. Taylor (Cambridge University Press).—Travels in India by Jean Baptiste Tavernier, 2 vols.: V. Ball (Macmillan).—Results of Meteorological Observations made in New South Wales during 1887; H. C. Russell (Sydney, Potter).—Ethnographische Beiträge zur Kenntniss des Karolinen Archipels: J. S. Kubary (Leiden, Trap).—Les Animaux et les Végétaux Lumineux: H. Gadeau de Kerville (Paris, Baillière).—Bibliographie Générale de l'Astronomie, tome premier, 2nde partie: J. C. Houzeau and A. Lancaster (Bruxelles, Hayez).—The Evolution of Sex, Prof. P. Geddes and J. A. Thomson (Scott).—Synthèse Scientifique et Philosophique: A. H. Simonin (Paris, E. Leroux).—The State: W. Wilson (Boston, Heath).—Notes on Sport and Ornithology: late Crown Prince Rudolf of Austria; translated by C. G. Danford (Gurney and Jackson).—Blackie's Geographical Manuals; No. 2, the British Empire; Part 1, The Home Countries: W. G. Baker (Blackie).—Gold-Fields of Victoria; Reports of the Mining Registrars for the Quarter ended June 30, 1889 (Melbourne).—Victoria; Annual Report on the Working of the Registration and Inspection of Mines and Mining Machinery Act during the Year 1888 (Melbourne).—Magnetism and Electricity, Advanced and Honours Questions: A. Jamieson (Griffin).—Electrical Engineering, Ordinary and Honours Questions: A. Jamieson (Griffin).—Results of Rain, River, and Evaporation Observations made in New South Wales during 1888: H. C. Russell (Sydney, Potter).—Astronomical and Meteorological Workers in New South Wales, 1778–1860: H. C. Russell (Sydney, Potter).—The Thunderstorm of October 26, 1888: H. C. Russell.—On a Self-recording Thermometer: H. C. Russell.—President's Address by H. C. Russell at the First Meeting of the Australian Association.—The Source of the Underground Water in the Western Districts: H. C. Russell.

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